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INFLUENCE OF LOUIS ON AMERICAN MEDICINE.*

By William Osler, M. D.

Harvey and Sydenham, types of the scientific and the practical physician, though contemporaries, were uninfluenced, so far as we know, by the other's work or method. Harvey had little reputation as a practical physician, and Sydenham cared little for theories or experiment. Modern scientific medicine, in which these two great types meet, had its rise in France in the early days of this century. True, there had lived and worked in England the greatest anatomist and medical thinker of modern times; but John Hunter, to whose broad vision disease was but one of the processes of nature to be studied, was as a voice crying in the wilderness to the speculative, theoretical physicians of his day.

Bichat's *Anatomie Générale* laid the foundation of the positive or modern method of the study of medicine, in which theory and reasoning were replaced by observation and analysis. Laennec, with the stethoscope, and with an accurate study of disease at the bedside and in the post-mortem room, almost created clinical medicine as we know it to-day.

The study of fevers occupied the attention of all the great physicians of the time. Fever—what it was, how it should be treated. What a vast literature exists between Sydenham and Broussais! What a desolate sea of theory and speculation!

No one had been more influenced by Bichat's brilliant teachings than Broussais, who ruled supreme in the medical world of Paris in the early decades of this century. A strong believer in careful observations at the bedside and in the post-mortem room, he was led into hopeless error in attributing fevers and many other disorders to irritation in the stomach and intestines—his gastro-enteritis.

Writing in the American Medical Recorder, July, 1821, an American student, Dr. F. J. Didier, says of the Paris professors of that date, "They were always talking of Hippocrates, Galen, Celsus, etc., as if not a particle had been added to the stock of knowledge since their time." And again, "The doctrines of John Brown, mixed up with the remnants of humoral pathology, form the basis of the present system."

The same mixture prevailed early in the fourth decade, as you may see from Broussais' Pathology, the American edition of which was issued in 1832, and from Jackson's (Samuel) Principles of Medicine, published in the same year.

Upon this scene, when Broussais was at the height of his fame, came Louis. He, with his friends Andral and Chomel, were very important factors in substituting finally in the study of medicine, for speculation and theory, observation and method.

The chief facts in Louis' life may be thus briefly stated. He was born in 1787 at Aï. He began the study of law, but abandoned it for that of medicine. He seems not to have

*Read before the Stillé Society of the Medical Department of the University of Pennsylvania.

been of a very strong constitution, as he did not pass the inspection for military service. He began the study of medicine at Rheims, and completed his course in Paris, where he graduated in 1813, in the twenty-seventh year of his age. While waiting at home, hesitating what he should do, M. le comte de Saint-Priest, who occupied an official position in Russia, happened to stay for a few hours in the town of Aï to see Louis' family, and it was suggested that the young physician should accompany him to Russia. He consented and in St. Petersburg obtained a diploma to practice. For three years he seems to have had no settled abode, but wandered about with his friend, who was Governor of one of the provinces. He then settled in Odessa, where he remained for four years and practiced with great success. In the last year of his stay in Odessa he was very much disturbed by the high rate of mortality in children with diphtheria, and this appears to have determined him to abandon for a time the practice of medicine and to devote himself to study. With this object in view he returned to Paris and for six months attended the practice at the Children's Hospital. Among the younger physicians in Paris he found an old fellow-pupil, Chomel, physician to La Charité, who offered him opportunities for work in his wards. Louis at this time was thirty-four years of age. Here for six years uninterruptedly he set himself to work to study disease in the wards and in the post-mortem room. At first he appears to have occupied the position simply as a voluntary assistant and friend of Chomel, but subsequently he became his *chef-de-clinique*, and during this period he occupied a room in the entresol of the hospital. He was a voluminous note-taker and collected in this time an enormous number of important facts.

This remarkable feature in Louis' life has scarcely been dwelt upon sufficiently. I know of no other parallel instance in the history of medicine. It is worth while reading the brief extract from Dr. Cowan's introduction to his translation of the work on Phthisis. "He entered the hospital of La Charité as a *clinical clerk*, under his friend, Professor Chomel. For nearly *seven years*, including the flower of his bodily and mental powers (from the age of thirty-three to forty), he consecrated the whole of his time and talents *to rigorous, impartial observation*. All private practice was relinquished, and he allowed no considerations of personal emolument to interfere with the resolution he had formed. For some time his extreme minuteness of inquiry and accuracy of description were the subjects of sneering and ridicule, and *cui bono?* was not infrequently and tauntingly asked. The absence of any immediate result seemed for a time to justify their contempt of a method involving too much labor and personal sacrifice to be generally popular or easily imitated; and M. Louis himself, at moments, almost yielded to the increasing difficulties of the task he had undertaken. No sooner, however, were his facts sufficiently numerous to admit of numerical analysis than all doubt and hesitation were dissipated, and the conviction that the path he was pursuing could alone conduct him to the discovery of truth became the animating motive for future perseverance. Many of the results to which he arrived soon attracted general attention, and among those who had formerly derided his method while they admired his zeal, he found many to applaud and a few to imitate. From this moment may be dated the presence of that strong impression of the necessity of exact observation by which the school of Paris has been since so distinguished, and which is now gradually pervading the medical institutions of the continent and our own country; it is undoubtedly to the author of the present volume that we ought to ascribe the practical revival of that system, which had for ages been verbally recognized but never before rigorously exemplified."

The following works appeared as a direct result of his studies during these six years:*

"In 1823, a memoir on perforation of the small intestines, in acute diseases; a second, on croup in the adult; a third, on the communications between the right and left cavities of the heart (Archives de médecine).

"In 1824, two memoirs on the pathological anatomy of the mucous membrane of the stomach; another on pericarditis.

"In 1826, a memoir on abscess of the liver; another on the condition of the spinal marrow in Pott's disease; a third on sudden and unforeseen deaths; a fourth upon slow but anticipated deaths, but which anatomy will not explain; a fifth on the treatment of tænia by the Darbon potion (Archives de médecine).

"In 1825, his Anatomical Researches, etc., on Phthisis (1 vol. 8vo); reprinted with many additions in 1843.

"In 1828, Researches on the Typhoid Affection or Fever (2 vols. 8vo); reprinted with many additions in 1841."

Louis introduced what is known as the Numerical Method, a plan which we use every day, though the phrase is not now very often on our lips. The guiding motto of his life was "Ars medica tota in observationibus," in carefully observing facts, carefully collating them, carefully analyzing them. To get an accurate knowledge of any disease it is necessary to study a large series of cases and to go into all the particulars —the conditions under which it is met, the subjects specially liable, the various symptoms, the pathological changes, the effects of drugs. This method, so simple, so self-evident, we owe largely to Louis, in whose hands it proved an invaluable instrument of research. He remarks in one place that the edifice of medicine reposes entirely upon facts, and that truth cannot be elicited but from those which have been well and completely observed.

American medicine felt the influence of Louis through two channels, his books and his pupils. Let us speak first of the former. No French writer of the century has had such a large audience in this country; all of his important works were translated and widely read. The work on phthisis, the first important outcome of five years' hard work at La Charité in Chomel's wards, was published in 1825. Much had already been done by physicians of the French school on this subject. Bayle's important *Recherches* had been issued in 1810, and Laennec had revolutionized the study of phthisis by the publication of his treatise on auscultation. I cannot enter into any detailed analysis of the work, but it is one which I can commend to your notice as still of great value, particularly as

*Brief Memories of Louis and some of his Contemporaries. H. I. Bowditch, Boston, 1872.

a model of careful observation. The work was based upon the study of 123 cases observed in Chomel's clinic. The lesions observed at autopsy are first described under the different organs, with great accuracy and detail, and then summarized, following which is an elaborate description of the symptomatology. I do not know of any single work on pulmonary tuberculosis which can be studied with greater profit to-day by the young physician. The fifty years which have elapsed since its publication, and the changes which have taken place in our ideas of tuberculosis, diminish naught from the value of his careful anatomical and clinical presentation of the subject.

In 1829 appeared his second great work, *Anatomical, Pathological and Therapeutical Researches upon the disease known under the name of gastro-enterite, putrid, adynamic, ataxic, typhoid fever, etc., compared with the most common acute diseases.* It was based upon 138 observations made between 1822 and 1827. He analyzed and determined the lesions found in fifty patients who had died of the typhus fever, and compared these with alterations found in other acute diseases. Altogether for this work he states that he analyzed the changes in the viscera of 133 subjects and the symptoms of nearly 900. In his introduction to this work he quotes a sentence from Rousseau which is always to be kept in mind: "I know that truth lies in the facts, and not in the mind that judges of them, and that the less I introduce what is merely my own into the deductions I make from them, the more certain I shall be of approaching the truth." This work was translated by Dr. H. I. Bowditch in 1836. At the time of Louis' observations, although differences were recognized between the various forms of continued fevers, the profession had no accurate knowledge of the subject. It so happened that at this period the disease prevailing at Paris known as typhus was almost entirely what we now call typhoid fever, so that the anatomical lesions found by Louis in his fifty autopsies were chiefly in the intestines; in all the Peyer's glands were diseased. His method was to analyze carefully the appearances found in the different organs in the series of fever cases, and compare them with patients who had died of other acute diseases; thus of course the contrast was striking in the very matter of involvement of Peyer's glands, which were more or less seriously changed in structure in all of the patients with the fever, while in the persons dead of other acute diseases the elliptical patches had no special redness or softening.

The symptomatology was also given in great detail, and the same painstaking comparisons were instituted between the subjects of the typhoid affection and those of other acute diseases. Louis' work convinced a majority of the members of the Paris school that the essential lesions in continued fevers were in the intestines, and Louis himself appears not to have had any idea whatever that the disease which he was studying was in any way different from the disease prevailing in other parts of Europe and which we now know as typhus fever.

The next important memoir, the essay on Blood-letting, had a very potent influence on professional opinion in this country. It appeared in Paris in 1835 and was translated by G. C. Putnam, with an introduction and appendix by Dr. James Jackson. As this learned physician remarks in his preface, "If anything may be regarded as settled in the treatment of disease, it is that blood-letting is useful in the class of diseases called inflammatory, and especially in inflammations of the thoracic viscera." When one reads the reports of the treatment by bleeding up to about the year 1840, one is almost forced to ask the question, are the diseases the same? or surely the patients must have possessed much more powerful constitutions than those which we are called upon to treat at the end of the century.

At the time of Louis' return to Paris, under the influence of Broussais' doctrine of irritation, local and general blood-letting was practised more extensively than at any previous period in the history of medicine. As an interesting illustration it may be mentioned that the trade in France and Spain in leeches had developed to proportions which assumed really those of a national industry, and even in this country I believe one of the medical societies offered a prize for the best demonstration of the practical method of cultivating leeches for medicinal purposes.

It must have been a terrible shock to Broussais and his adherents when Louis attacked the subject of blood-letting in pneumonia with his numerical method. For this purpose he analyzed 78 cases, 28 of which proved fatal, and in a second series 29 cases with 4 deaths. Among his conclusions were that pneumonitis is never arrested at once by blood-letting, and that the supposed happy effect on the progress of the disease was very much less than was commonly believed. Incidentally he remarks with reference to the practice of blistering which was in vogue at the time, that he had rejected the practice after the treatment of 140 cases of pleurisy without losing a case. I would refer you particularly to Putnam's translation of this article, which you can obtain in any of the libraries, not only for Louis' work, but for the excellent introduction by Dr. Jackson on the value of the numerical method in medicine, and also for the appendices, analyzing the pneumonia cases of the Massachusetts General Hospital from 1824 to 1834 (inclusive).

To American students one of Louis' most valuable works is his Research on the Yellow Fever in 1828. On the 1st of November, 1828, Louis, with Chervin and Trousseau, left for Gibraltar, where the disease prevailed. They made a very careful study of the symptoms and morbid anatomy, and on their return to Paris made a report to the Academy of Medicine, but the work remained in manuscript until Dr. Geo. C. Shattuck translated it into English and it was published by the Massachusetts Medical Society as Vol. X of their Library of Practical Medicine. The work did not appear in French until 1844. It is chiefly valuable as a very accurate and careful record of a series of cases studied clinically and anatomically.

Powerful as was the effect of Louis' writings on American medicine, it cannot compare with the influence which he exerted through his pupils, who "caught his clear accents, learned his great language, made him their model." Of the great triumvirate of the French school of the fourth decade, Louis possessed a singular power of attracting hard-working, capable men, and this in spite of the fact that his rivals and friends, Chomel and Andral, possessed more brilliant gifts of

a certain kind. As a writer in the Lancet said (1872, II), "Year by year fresh bands of students came to imbibe from his lips the instruction which their predecessors had abandoned with reluctance, till his academic progeny knew no distinction of race or even color, but coalesced into a noble band of enthusiasts in the cause of medicine, of science and of humanity." In this academic progeny Louis' American pupils take a very unusual position. Among the thousands in the profession of this country who have during this century sought light and learning in the older lands, the group of young men who studied in Paris, between 1830 and 1840, had no predecessors and have had no successors. Partly because the time was ripe and they were active agents in bringing the new art and science to the New World, partly owing to inherent capabilities, etc., but not a little because the brightest minds among them fell under the influence of Louis—they more than any others gave an impetus, which it still feels, to the scientific study of medicine in the United States.

There had been, of course, in Paris many students from this country prior to 1830, but they do not form a school, recognizable to us at present. One name comes to my mind, that of the Rhode Island philosopher, Elisha Bartlett, a peripatetic of the peripatetics, in the days when men moved from city to city, like the Sophists of ancient Greece. I do not know whether when in Paris in 1828 he came personally under Louis' influence—probably not, as Louis spent part of that year in Spain—but he brought back recent French methods, with Gallic lucidity and a keen appreciation of the value of the numerical method. His well known work on Typhus and Typhoid Fever, issued in 1842, is in itself a lasting witness to the intelligence and progressive character of the younger teachers of that day. With a clear separation of Typhus, Typhoid, the Periodic and Yellow Fevers, it had at the date of its publication no counterpart in European literature, and is in remarkable contrast to the chaotic treatises of Armstrong, Fordyce, Tweedie, Southwood Smith and others.

Without attempting to give a complete list, the following were among the American students in Paris between 1830 and 1840:

From Boston, James Jackson, Jr., H. I. Bowditch, Oliver Wendell Holmes, Geo. C. Shattuck, Jr., John D. Fisher, J. C. Warren (then past middle age), and J. Mason Warren.

From New York, John A. Swett, Abraham Dubois, Alonzo Clark, Charles L. Mitchell, Charles D. Smith, Valentine Mott, Sr., and John T. Metcalf.

From Philadelphia, Geo. W. Norris, W. W. Gerhard, Casper W. Pennock, Thomas Stewardson, Alfred Stillé, Thomas D. Muter, J. Campbell Stewart, Charles Bell Gibson, John B. Biddle, David H. Tucker, Meredith Clymer, Wm. P. Johnston, W. S. W. Rushenberger, Edward Peace, William Pepper, Sr.

From Baltimore, William Power.

From the South, Peter C. Gaillard, Gibbs, and Peyre Porcher of Charleston; J. L. Cabell, L. S. Joynes, Selden and Randolph of Virginia.

"And many more whose names on earth are dark"—men of the stamp of Dr. Bassett of Alabama, who felt the strong impulsion to know the best that the world offered, every one of whom has left a deep and enduring impression in his sphere of work.

It would be impossible to tell in detail how Louis' students brought back his spirit and his methods to their daily work, and of the revolution which they gradually effected in the study and in the treatment of disease. I can best, perhaps, fulfill my object by referring somewhat fully to two of the most distinguished among them, James Jackson, Jr., and W. W. Gerhard.

James Jackson, Jr., is the young Marcellus among the physicians of this country, "the young Marcellus, young, but great and good." I do not know in our profession of a man who died so young who has left so touching a memory. He was the son of Dr. James Jackson, of Harvard, one of the most distinguished of New England's physicians, a man to whom our generation owes a heavy debt, since he, with Jacob Bigelow, was mainly instrumental in bringing about more rational ideas on the treatment of disease. Of Louis' pupils from this side of the water, young Jackson seems to have been his special favorite. After taking the B. A. degree at Cambridge in 1828, Jackson attended the medical lectures at Harvard, and in the spring of 1831 went to Paris, where he remained until the summer of 1832. Returning home in 1833, he graduated in medicine at Harvard in 1834. In the two years and a half of his studies in this country before going abroad he had had exceptional opportunities with his father at the Massachusetts General Hospital, and showed his early industry and ability by taking one of the Boylston Prize Essays before the completion of his second year of study.

In Paris he attended the practice of La Pitié and St. Louis. He soon became devoted to Louis, and by him was utilized to the full in the cholera epidemic in 1832. Two letters from Louis to James Jackson, Sr., show how important he thought a prolonged period of study was for a young man. He says: "I pointed out to him (James Jackson, Jr.) the advantage it would be for science and for himself if he would devote several years exclusively to the observation of diseases. I now retain the same opinion and am strengthened in it; for the more I become acquainted with, and the more I notice him applying himself to observation, the more I am persuaded that he is fitted to render real service to science, to promote its progress. I find that he would be well pleased to follow for a certain period the vocation for which nature has fitted him; but he has stated to me that there are many difficulties which would prevent his devoting himself exclusively to observation for several years. But can these difficulties be insurmountable?"

And again: "Let us suppose that he should pass four more years without engaging in the practice of medicine, what a mass of positive knowledge will he have acquired! How many important results will he have been able to publish to the world during that period! After that he must necessarily become one of the bright lights of his country; others will resort to him for instruction, and he will be able to impart it with distinguished honor to himself. If all things be duly weighed, it will appear that he will soon redeem the four years, which men of superficial views will believe him to have lost." In another letter, the following year, just before young

Jackson's departure from Paris, he refers again to this question and urges Dr. Jackson to allow his son to devote himself exclusively to observation for several years in Boston. The extract from this letter is worth quoting. "Think for a moment, sir, of the situation in which we physicians are placed. We have no legislative chambers to enact laws for us. We are our own lawgivers; or rather we must discover the laws on which our profession rests. We must *discover* them and not invent them; for the laws of nature are not to be invented. And who is to discover these laws? Who should be a diligent observer of nature for this purpose, if not the son of a physician, who has himself experienced the difficulties of the observation of disease, who knows how few minds are fitted for it, and how few have at once the talents and inclination requisite for the task? The inclination especially, for this requires that the observer should possess a thorough regard for truth, and a certain elevation of mind, or rather of character, which we rarely meet with. All this is united in your son. You ought—for in my opinion it is a duty—you ought to consecrate him for a few years to science. This, sir, is my conviction, and I hope it will be yours also. I know very well that every one will not be of the same opinion; but what matters it, if it be yours?—if you look upon a physician, as I do, as holding a sacred office, which demands greater sacrifices than are to be made in any other profession."

Young Jackson's letter to his father, just as he was quitting Paris, indicates on what affectionate terms he had lived with Louis. "In two hours I am out of Paris. I will not attempt to describe to you the agony it gives me to quit Louis. He is my second father, and God knows that is a name I of all men cannot use lightly. I may not persuade you to look upon him with my eyes exactly as a scientific man; but in your heart he must have the share of a brother; for he almost shares my affection with you. From one upon whom I had no claims but those which my life and mind and habits gave me, I have experienced a care, an affection which I never could dare expect from any but my dear father, and which I shall ever feel to be the most honorable and truly worthy prize of my life."

He seems to have inspired the same tender feelings in all his American students. In the *Memoir* of Dr. Bowditch, to which I have already referred, he speaks of Louis' fatherly kindness to him during a prolonged attack of rheumatic fever lasting for many weeks.

Young Jackson was one of the founders, in 1832, of the Society for Medical Observation, which consisted of the ablest of the students of Louis, Chomel and Andral. During his stay in Paris he made an important study of cholera, which was published in this country in 1832. It was most timely, as it gave the profession here a very clear and accurate description of the disease, of which up to that time they had had no experience. Jackson's name, too, will always be associated with the studies upon emphysema, and he is the discoverer of the prolonged expiration in early pulmonary tuberculosis.

Returning to Boston in the autumn of 1833, he spent the winter preparing for his degree and elaborating the notes which he had taken in Paris. In March he fell ill with a dysentery, which proved fatal on the 27th of the month, in the twenty-fifth year of his age. I know of no young man in the profession who had given pledges of such exceptional eminence. His influence in extending Louis' methods and views throughout New England was chiefly through his father, who, though a man approaching his sixtieth year, became an ardent follower of Louis and the numerical method.

In Oliver Wendell Holmes' recently issued Biography you will find a delightful description of life at the Medical School of Paris at this period. He bears witness to the good effect which Jackson's warm friendship with Louis had had in promoting the interests of American students. I may conclude with a quotation from Dr. Jackson's, Sr., memoir: "At the suggestion and request of one of my most judicious brethren I shall add that my son's influence on the profession here, in the short time he was with us, was of a very salutary description. This gentleman states that my son not only caused others, who had not yet read the works of M. Louis, to study them with care, but that he induced among the rising members of the profession in our own city the habits of thorough observation of the phenomena of disease in the living and in the dead, which he had learned from the same great pathologist. He also taught us much in respect to the physical signs of disease in the thorax, with which we were imperfectly acquainted before; at least I may say this was true as to myself. Indeed I ought to say more, for he aided me very much in regard to the diagnosis of the more obscure diseases of that region, derived from the combination of the physical and rational signs. On emphysema of the lungs he threw, for me, quite a new light."

Wm. W. Gerhard was the most distinguished of the American pupils in Paris between 1830 and 1840. When you call to mind the men whom I have mentioned, this may seem a strong statement, but I feel certain that could we take their suffrages they would accord him the place of merit in consequence of the character of his work. Dr. Gerhard was born in Philadelphia, in 1809, and was graduated from the University of Pennsylvania in 1831. Early in the year he went to Paris and attached himself to Louis at La Pitié. In one of his letters* to his brother, dated January 18, 1832, he says: "Dr. Louis is delivering an interesting clinic at La Pitié; he is a remarkable man, very different from the physicians of England or America, and remarkable even at Paris by the strict mathematical accuracy with which he arrives at his results; he is not a brilliant man, not of the same grade of intellect as his colleague at La Pitié, Andral." In another letter he gives an account of his day's work. "The morning from seven to ten is occupied with the visit and clinic at the hospital; there are several distinct clinics now in actual progress; each of them has its advantages. I shall vary my attendance at the different hospitals and select those lecturers who are of real merit. At this moment we are following ing Piorry at the Salpetrière, a very distant hospital, two or three miles from our lodgings; his patients are all old women, and not interesting. My object in following his course is to obtain some interesting information on the best mode of

*I am indebted to members of Dr. Gerhard's family for the letters from which these extracts are taken.

investigating the diseases of the chest. M. Piorry has devoted special attention to this subject. From Salpetrière we hurry to La Pitié; we hear a surgical lecture, reach home to breakfast, and then to the school of medicine The lectures at the school, with a private course of anatomy during the hour of intermission, fill up the remainder of the day until four. Fortunately a private clinic at La Charité introduces me to a set of very interesting cases, especially on pectoral cases. Dr. Dagneau has a class who pay him ten francs a month and enjoy the privilege of examining the patients much more conveniently than is practicable during the morning visit in the midst of a crowd of students. We dine at five-thirty and then lectures again until eight o'clock. Imagine the facilities, the delightful advantage of acquiring positive information, and what is at least as important, of learning the mode of obtaining these positive results. We see and hear the men who are so well known to us in America, learn to form a correct estimate of their relative worth—in short, one of the most striking advantages of a medical visit to Europe is to acquire the sort of liberal professional feeling which is rarely secured by the continued intercourse with the same men, and the unpleasant medical politics which divide the profession in America."

Evidently Broussais made no special impression on Dr. Gerhard. He says, "Broussais is the best known, his reputation is universal, and the benefits he has conferred on medicine are immense, but unfortunately he is a wretched lecturer. His own opinions are given in the most awkward, clumsy manner; the manner and style of lecturing are coarse and vulgar."

In another letter of February 3, 1832, he tells how he induced Louis to give them private instruction. To his brother he writes: "I must write you at least a few days before the excitement has passed off: can you imagine how fortunate I am—devinez si vous pouvez—two or three days ago, Jackson, Pennock and myself were talking of hospitals and morbid anatomy, when the idea occurred of attempting the study of pathology in a particular manner. It was this: to obtain the specimens and study them, the authors in our hand, exactly and carefully comparing authorities with the subject before us. We addressed ourselves to two of the *internes* at La Pitié attached to the *salles* of Louis and Andral, and they agree to procure all facilities in their power and communicate their own information for the compensation of 60 francs from each of us; we accordingly visit La Pitié on three afternoons of the week and examine the parts at the hospital, afterwards carrying home such portions as require minute investigation. Our first success in this opening of new sources of instruction emboldened us to attempt something of higher importance. We were all desirous of studying auscultation, of studying it in such a manner as to be sure of our ground on our return, and to be capable of appreciating the advantages of the art. Louis' public instructions were valuable, but his private lessons upon a subject demanding minute and patient inquiry we knew would be infinitely more so. I therefore in the name of my friends addressed him a polite note, accompanied by a handsome pecuniary offer; we did this with little hopes of success, but happily for us he accepted our proposition, and next week we are his private pupils at La Pitié. We are, I believe, the first who have made this arrangement with M. Louis, and you may estimate its importance when I tell you that he is considered in excellence of diagnosis the successor of Laennec. Our advantages for the study of pathology and the diagnosis of diseases of the chest are now superior; they are indeed the very best in the world, and our eagerness to embrace them will, I hope, render them of real utility; of course they involve an additional expenditure of 400 or 500 fr., but I should be happy to shorten my stay at Paris a month to improve the remainder of my time in this manner, if such were necessary for me. Pennock and myself are very happy to have become intimate with Jackson; he has superior talents, and his excellent education, conducted by his father, unquestionably the first physician in America, has cultivated his mind and developed an ardent attachment to medicine."

Few American students have occupied their time abroad to greater purpose than Dr. Gerhard. He appears to have been an indefatigable worker, and the papers which he published based upon material collected in Paris are among the most important which we have from his pen. Thus with Pennock he described Asiatic cholera in 1832. Devoting himself particularly to the study of diseases of children, he issued a very interesting paper on small-pox, and two papers of very special value, the first on tuberculous meningitis and the other upon pneumonia in children. Both of these papers mark a distinct point in our knowledge of these two diseases. He is usually accorded the credit of the first accurate clinical study of tuberculous meningitis.

Late in the year 1833 he returned to Philadelphia, and at his suggestion his friends had secured him the appointment as resident physician at the Pennsylvania Hospital, which he took early in 1834. This step indicated how carefully he had weighed the important influence in Louis' career of the years of quiet work at La Charité. At the Pennsylvania Hospital he had an opportunity to study the common continued fever of the country, and determined that it was identical, clinically and anatomically, with the typhoid fever of Louis, and characterized by a special lesion in the glands of Peyer. I do not know exactly how long he remained resident physician at the Pennsylvania Hospital, but he was soon after appointed one of the physicians at Blockley, and here in 1836 he was able to carry out his most important piece of work. The general opinion prevailed that the fever which Louis described and which had the lesions in the small bowel was only a modification of the ordinary typhus fever which at that time prevailed so extensively, particularly in Great Britain and Ireland. In London, Edinburgh and Dublin the intestinal lesions were regarded as only accidental, and not indicative of a special affection. Dr. Gerhard knew the typhoid fever of Louis well, and had had an opportunity of studying it again at the Pennsylvania Hospital, so that when the epidemic of typhus fever developed in 1836 he was in a very good position to make an accurate study of the disease. Two hundred and fourteen cases were observed, and as a result of his study he declared positively that the typhus fever, which was similar to the disease which he had also seen in Edinburgh, was a different affection altogether from the typhoid fever with intestinal lesions. These observations, you must remember,

were made in 1836, at a time when the greatest confusion existed as to the forms of fever. It took a great many years in Great Britain before the duality of the prevalent fever was recognized, but owing to the influence of Gerhard's paper, and to the accurate knowledge of fever brought to this country by Louis' pupils, the differentiation of the two diseases was here quickly recognized, since, as already mentioned, Bartlett in 1842 considered them apart.

Gerhard's work influenced his Paris friends greatly, and this was strengthened by the papers read before the Society for Medical Observation by Geo. C. Shattuck and Alfred Stillé, of whom the former had had opportunities of studying typhus fever in Great Britain, while the latter had been one of Gerhard's house physicians in the typhus epidemic at Blockley. Shattuck's paper is published in the Medical Examiner for 1840. I have always regretted that Dr. Stillé's paper has never appeared in print. He was kind enough to let me see it, and, as I have mentioned elsewhere, the differential points between typhus and typhoid fever are nowhere more clearly laid down.

The University of Pennsylvania early took advantage of Gerhard's training and utilized him as clinical lecturer at the Philadelphia Hospital. He soon acquired a special reputation in diseases of the heart and lungs. In 1842 appeared the first edition of his work on Diseases of the Chest, which ran through four editions, and is still a valuable work of reference. One of his fellow-students in Paris, Stewardson, has given a very pleasing picture of him as a clinical teacher: "As a clinical teacher he was remarkably successful and exerted a powerful and commanding influence. Without any pretension to eloquence, he nevertheless riveted the attention of his hearers and stimulated their enthusiasm. Himself deeply interested in his subject, he communicated this interest to his audience by the sheer force of truth. Students saw that truth was his object, not display; the advancement of science, and not the gratification of personal feelings, whether of vanity or ambition; in short, that in his mind, a deep interest in his subject and a thorough conscientiousness in the pursuit of it were the overmastering motives. In an easy and conversational style he presented to his hearers a graphic portraiture of the case before them, bringing into relief its most important symptoms; impressing upon their minds the most striking features in its history; pointing out, by a few clear and practical expressions, the bearing of any particular fact upon interesting medical questions, but avoiding long and labored arguments, or general disquisitions upon the nature of diseased action. He neither stimulated the fancy by the flowers of rhetoric, nor amused the intellect with episodes upon theoretical questions, but confined himself to drawing such practical conclusions as were clearly deducible from the facts presented. No man of his day enjoyed so high a reputation as a clinical teacher, and not only did he succeed in an eminent degree in arousing the enthusiasm of students and putting them in sympathy with himself, by infusing into them his own ardor in his favorite study; but he produced an influence upon the profession here which is felt still, which has fostered the establishment of clinical teaching among us, and done much to give it that rank which it now occupies here as a branch of medical instruction."

Of the work of Louis' other students in this country time would fail me to tell—of the influence of Bowditch, Holmes and Shattuck in Boston, of Swett, Clark and others in New York, of Pennock, Stewardson, Stillé in Philadelphia, and of Power in Baltimore. To them all we owe a heavy debt of gratitude. They brought from Paris enthusiasm, faith in the future, faith in the profession of their choice, accurate methods and a loyal love of truth. Endowed with the spirit and zeal of their master, they carried his great message to the New World; and more than this, touched with those finer qualities which made Louis so lovable, they have become bright ideals for all future generations of American students.

There remain, so far as I know, three only of the Paris students of whom I have spoken, John T. Metcalf, Meredith Clymer, and your honored patron, Alfred Stillé. They, too, must soon go the way of all the earth; but among the consolations of old age what greater solace can they feel than that the lives of the men whose fathers and grandfathers they taught are still made better by their presence.

WILLIAM HARVEY AS AN EMBRYOLOGIST.*

By William K. Brooks, LL. D., *Professor of Zoology, Johns Hopkins University.*

The immortal discoverer of the circulation of the blood is held to be also the discoverer of the law of embryology—"that all animals are produced out of ova" (Encyc. Brit., 9th ed., art. Embryology, p. 164); and he is also held to have had some vague premonition, scarcely worth mentioning in history, of the great law that the complex animal arises, from a relatively homogeneous germ, by gradual differentiation or epigenesis.

I hope to show, by quotations from his work on embryology (Exercitationes de Generatione Animalium, Amstelodami, 1651, translated into English by Robert Willis, M. D., London, 1848), that both these current impressions are erroneous. He not only formulated but demonstrated epigenesis. His statement of this law is clear, definite and thoroughly modern, and it is based upon actual observations which are fully described.

On the other hand the conception which he sought to express by the dictum "omne vivum ex ovo" is totally foreign to the principles of modern embryology. Harvey was a worker, not a dreamer, and his dictum is no mere guess or happy inspiration. It sums up results reached by laborious research, and as a generalization based on actual study it still has value,

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although its meaning has nothing in common with that which the words have as we now use them. He repudiates in most energetic language the opinions, current at his time, which come nearest to the modern discovery that the physical continuity of living matter is never broken. In fact the chief aim of his treatise is to show that his observations, as he interpreted them, prove that there is no physical or "corporeal" continuity between parent and child.

Embryologists who permit Germans to write the history of their science, and make no protest when the demonstration that the embryo arises from the egg by epigenesis is attributed to Wolff (1759), and to Von Baer (1829), are either ignorant of Harvey's researches (1651) or indifferent to the fame of this great Englishman, who studied the history of the chick as laboriously and faithfully as Von Baer, nearly two hundred years before. While his resources were more limited, his ability to reflect upon the meaning of his observations and to state in clear and energetic words the results of his "Beobachtung und Reflexion," were inferior in no way to those of the justly famous author of the "Entwickelungsgeschichte der Thiere."

Harvey means just what more modern writers mean by "epigenesis," but the strangeness of the views he opposed gives us difficulty. The form of words into which an account of a scientific discovery falls is fixed by the view of the matter which is current at the writer's day, and later generations of readers may be puzzled by inability to occupy his standpoint. Thus it is with Harvey, and we thus explain the prevalence of the opinion that he had no more than a dim adumbration of truths the demonstration of which is generally credited to Wolff and Von Baer.

The evolutionary teachings of Bonnet are quite intelligible to us; and as we easily put ourselves in Von Baer's place, his refutation of Bonnet appeals to us with all its native force; but it is much harder for us to stand where Harvey stood.

So far as I can discover, no notion at all equivalent to Bonnet's conception of germs ever entered Harvey's mind or the mind of any one before his time. He presents the evidence for epigenesis as opposed, not to "evolution," but to "metamorphosis," and his way of using the last word is so unfamiliar to us that we cannot grasp what he has in mind without effort.

They who studied embryology before him held one modification or another of the very ancient belief that embryos arise from "excrement"; that they are products of decomposition.

He gives the evidence for epigenesis as opposed to this opinion which finds no pigeon-hole in the modern mind. Fortunately he is a ready writer. Illustrations and analogies overflow his brain and pen; and patient study enables us to pick out passages which give his views on epigenesis uncomplicated by reference to "metamorphosis." When we have done this we find his reasoning as modern and definite as that of Von Baer, although his resources did not qualify him to sum up the evidence with modern exhaustiveness.

While Harvey does not deny that some "imperfect" animals may be generated "out of a putrescent material, the drying of a moist substance, or the moistening of a dry one," he tells us, clearly and definitely enough, that the generation of all "perfect" animals, such as the lion and the cock, "is the result of epigenesis as the man proceeds from the boy; the edifice of the body, to wit, is raised on the punctum saliens as a foundation; as a ship is made from the keel, and as a potter makes a vessel . . . For out of the same material from which the first part of the chick or its smallest particle springs, from the very same is the whole chick born; whence the first little drop of blood, thence also proceeds its whole mass by means of generation in the egg; nor is there any difference between the elements which constitute and form the limbs or organs of the body, and those out of which all their similar [*i. e.* homogeneous] parts, to wit, the skin, the flesh, veins, membranes, nerves, cartilages, and bones derive their origin. For the part that was at first soft and fleshy, afterwards, without any change in the matter of nutrition, becomes a nerve, a ligament, a tendon; what was a simple membrane becomes an investing tunic; what had been cartilage is afterwards found to be a spinous process of bone, all variously diversified out of the same similar [homogeneous] material." From what "appears to be homogeneous in the beginning and resembles the spermatic jelly" the structure of the body arises; its parts being "at first delineated by an obscure division, and afterwards become separate and distinct organs."

He says the result of the process of development is just as if the chick were created by a command to this effect: "Let there be a similar [homogeneous] colorless mass, and let it be divided into parts and made to increase, and in the meantime, while it is growing, let there be a separation and delineation of parts; and let this part be harder and denser and more glistening, that be softer and more colored."

"Now it is in this very manner that the structure of the chick in the egg goes on from day to day; all its parts are formed, nourished and augmented out of the same material. . . . For there is a greater and more divine mystery in the generation of animals than the simple collecting together, alteration and composition of the whole out of parts would seem to imply; inasmuch as here the whole has a separate constitution and existence before its parts, the mixture before the elements."

These passages summarize conclusions from observations which have been more fully described in forty-four preceding chapters or "exercises," and it would be difficult, even at the present day, to state in more definite language the truth that the developing embryo passes "from indefinite, incoherent homogeneity to definite, coherent heterogeneity by successive integrations and differentiations."

While we have no desire to ignore the merits of Wolff or to belittle the greatness of Von Baer, we find it hard to understand how any one who knows Harvey's works can, without protest, read this assertion or similar ones in the German works from which it is derived:

"It was reserved for Caspar Frederick Wolff, a German by birth . . . to bring forward observations which . . . established the theory of epigenesis upon the secure basis of ascertained facts" (Encyc. Brit., 9th ed., Embryology, 165).

We now know that the germ itself is an organism of wonderful complexity; that its homogeneity is relative, not absolute; but there is great mystery to us as well as to Harvey in the

manner in which "the whole has a separate constitution and existence before its parts," and while the doctrine of "metamorphosis" as held in Harvey's day has vanished from science, I venture to believe that we shall find in his discussion of this doctrine, clear statement of other difficulties which are still as grave as he found them.

It is hard to decide just what his opinion on spontaneous generation was. No less careful a student than Huxley tells us (Encyc. Brit., 9th ed., art. Evolution, p. 746) that "Harvey believed as implicitly as Aristotle did in the equivocal generation of the lower animals." "Harvey shared the belief of Aristotle—whose writings he often quotes, and of whom he speaks as his precursor and model, with the generous respect with which one genuine worker should regard another—that such germs may arise by a process of 'equivocal generation' out of non-living matter."

I am by no means confident that this assertion does justice to Harvey, or that the quotations from Aristotle prove anything except that Harvey was not yet quite prepared to demonstrate their error. I believe there is ample evidence that he had made many observations which, while he never published them, led him to distrust most of the familiar examples of spontaneous generation, although he may not have been fully armed to attack the teachings of "my leader," Aristotle, "one of nature's most diligent inquirers," "whose authority has such weight with me that I never think of differing from him inconsiderately." It is true that he quotes without comment, and occasionally without credit, many passages in which Aristotle affirms spontaneous generation; but as an offset to this he tells us explicitly (Exercise the forty-first) that he shall show in another place "that many animals, especially insects, arise and are propagated from elements and seeds so small as to be invisible (like atoms flying in the air), scattered and dispersed here and there by the winds; and yet these animals are supposed to have arisen spontaneously, or from decomposition, because their ova are nowhere to be found." He was far too cautious to have ventured to criticise "the philosopher," even to this extent, without pretty good evidence; and in Exercise the sixty-ninth he tells us why this evidence was never published. "Let gentle minds forgive me," he asks, "if, recalling the irreparable injuries I have suffered, I here give vent to a sigh. This is the cause of my sorrow:—whilst in attendance on his majesty the king, during our late trouble and more than civil wars, not only with the permission but by the command of the Parliament, certain rapacious hands stripped not only my house of all its furniture, but, what is subject of far greater regret with me, my enemies abstracted from my museum the fruits of many years of toil. Whence it has come to pass that many observations, *particularly on the generation of insects*, have perished, with detriment, I venture to say, to the republic of letters."

Is there not reason to believe that, if they are ever discovered, those lost observations will be found to cover some of the ground which was so successfully explored by Spalanzanni more than a hundred years later?

Harvey's reference (Exercise the twenty-seventh) to "the animalculæ which are engendered in our bodies . . . lumbrici, ascarides, lice, nits, syrones and acari," and to "the worms which are produced from plants and their fruits, as from gall-nuts, the dog-rose, and various others," might be held to imply belief in heterogenesis, if he did not tell us, almost immediately, that: "It certainly cannot be that the living principles of these animals which arise in the gall-nuts existed in the oak, although these animals live attached to the oak and derive their sustenance from its juices."

Notwithstanding Huxley's opinion, Harvey seems to have been nearer than any of his successors for a hundred years to the modern discovery that all living things come from germs, although I shall show soon that he did not intend to imply anything at all like the modern view by his statement that this is true.

In his discussion of epigenesis as contrasted with "metamorphosis," he assumes the reality of "equivocal" generation, as he does in many other places, although, in view of the passages I have quoted, I believe that this is admitted out of courtesy to Aristotle, and for the sake of the argument, as something which he is not yet fully prepared to disprove.

He tells us (Exercise the forty-fifth) that there are two ways in which one thing may be made out of another. When a workman cuts the material already prepared, divides it and rejects what is superfluous, till he leaves it in the desired shape, as in making a statue from a block of stone, the whole material of the future piece of work has already been in existence before it is finished into form or any part of the work is yet begun. When on the other hand a potter educes a form out of clay by the addition of parts, increasing its mass, and giving it a figure, at the same time that he provides the material, which he prepares, adapts and applies to his work, "the form may be said rather to have been *made* than *educed*." "So exactly it is with regard to the generation of animals. Some, out of a material previously concocted and that has already attained its bulk, receive their forms and transfigurations; and all their parts are fashioned simultaneously, each with its distinctive characteristics, by the process called metamorphosis, and in this way a perfect animal is at once born; on the other hand, there are some in which one part is made before another, and these from the same material afterwards receive at once nutrition, bulk and form; that is to say, they have some parts made before, some after others, and these are at the same time increased in size and altered in form. The structure of these animals commences from some one part as its nucleus and origin, by the instrumentality of which the rest of the limbs are joined on, and this we say takes place by the method of epigenesis, namely by degrees, part after part; and this is, in preference to the other mode, generation properly so called. In the former of the ways mentioned, the generation of insects is effected when by metamorphosis a worm is born from an egg; or out of a putrescent material, the drying of a moist substance or the moistening of a dry one, rudiments are created from which, as from a caterpillar grown to its full size, or from an aurelia, springs a butterfly or fly already of a proper size, which never attains to any larger growth after it is first born. But the more perfect animals with red blood are made by epigenesis or the superposition of parts. In the former, chance or hazard seems the principal promoter of gen-

eration, and the form is due to the potency of a pre-existing material; and the first cause of generation is 'matter' rather than an 'external efficient,' while the more perfect animals owe their immortality to one constant source—the perpetuation of the same species . . . Bees, wasps, butterflies, and whatever is generated from caterpillars by metamorphosis, are said to have sprung from chance, and therefore to be not preservative of their own race. . . . The lion and the cock owe their existence, as it were, to nature, or an operative faculty of a divine quality, and require for their propagation an identity of species, rather than any supply of fitting material." "In the generation by metamorphosis forms are created as if by the impression of a seal, or as if they were adjusted in a mould . . . but an animal which is created by epigenesis attracts, prepares, elaborates, and makes use of the material all at the same time. The processes of formation and growth are simultaneous. In generation by metamorphosis the whole is distributed and separated *into* parts, but in that by epigenesis the whole is put together out of parts, in a certain order, and constituted *from* them. In the one case the result is due to matter; in the other the animal makes itself.

"Now it appears clear from my history that the generation of the chick from the egg is the result of epigenesis rather than of metamorphosis, and that all its parts are not fashioned simultaneously, but emerge in their due succession and order; it appears, too, that its form proceeds simultaneously with its growth, and its growth with its form; also that the generation of some parts supervenes on others previously existing, from which they become distinct; lastly, that its origin, growth and consummation are brought about by the method of nutrition.

"The formative faculty of the chick rather acquires and prepares its own material than only finds it when prepared, and the chick seems to receive its growth from no other than itself. And as all things receive their growth from the same power by which they were created, so likewise should we believe that the chick is created by the same power by which it is preserved and caused to grow."

The meaning of this rather puzzling passage will be somewhat clearer after we have examined Harvey's views on generation, but when we omit the complications which come from the reference to "metamorphosis," its meaning as interpreted by the rest of the essay is about as follows:

The substance which composes the body of all "perfect" animals does not exist as such before the body itself is formed; but it consists of unorganized or "homogeneous" food which is changed by nutrition into all the diversified parts of the complicated body, so that nutrition, growth and development go on together. As the organized body is constructed by the assimilation of unorganized food, its structure cannot be the outcome of the ordinary or physical properties of this food. There must be some organizing influence at work making use of these properties to construct out of homogeneous matter a definite organism belonging to the same species with the parents. To the question what this organizing influence is, he answers that this is a "divine mystery," or, in plain English, that he does not know, although he finds clear evidence of its existence. He says in many places that the egg has a "vital principle," but the context shows that he means by this no more than we mean when we say it is "alive," and nothing is farther from his thoughts than recourse to supernatural agencies, for he tells us clearly that while the cause of its development is a "divine mystery," "the egg is a natural body endowed with animal virtues . . . it is moreover a body which under favorable circumstances has the capacity to pass into an animal form; heavy bodies, indeed, do not sink more naturally, nor light ones float, when they are unimpeded, than do seeds and eggs in virtue of their inherent capacity become changed into vegetables and animals." (Exercise the twenty-sixth, p. 272.)

It would be a gross error to infer, from this passage or from others like it, any further similarity between Harvey's opinions and the results of modern microscopic study of ova and male cells. In order to understand the meaning of his celebrated dictum "omne vivum ex ovo" we must undertake more extended analysis of his observations and reflections on generation, and of the opinions of his predecessors.

Aristotle saw nothing strange or exceptional in the generation of animals from decomposing organic matter, for he believed that all generation takes place in essentially the same way; and he regarded the generation of insects from putrescent slime as a simple or typical example, what we should now call a primitive type, of generation in general, in comparison with which more complicated instances are to be interpreted.

As a bloody substance is discharged at intervals from the reproductive organs of woman, during the fertile period of her life, and as its appearance marks the beginning and its cessation the end of fertility, he believed that the mammalian embryo is formed out of this substance just as other animals are generated from decomposing matter of other kinds.

"Milk and the menstrual discharge," he tells us (De Gen. II, 4), "are of the same nature." "When the semen masculinum enters the female uterus it coagulates the purest part of the catamenia," and when this has "set in the uterus" it forms a coagulum like curdled milk. As heat causes milk to curdle, so "the semen or geniture of the male bears the same affinity to the nature of the catamenia" and causes it to "set" without itself contributing any part of the substance of the coagulum. "The female always supplies the matter, the male the power of creation, and this it is which constitutes one male and another female." "The male is the efficient agent, and by the motion of his geniture creates what is intended from the matter contained in the female." "The body and the bulk therefore are necessarily supplied by the female; nothing of the kind is required from the male; for it is not even requisite that the instrument, nor the efficient agent itself, be present in the thing that is produced. The body then proceeds from the female; the life (anima) from the male."

Harvey points out the inconsistency of Aristotle's admission that hybrids "partake of the species of both parents" (De Gen. Anim. II, 2), and his assertion that "the conception or egg receives" "from the female its body solely and its dimensions," and that the mother has no part in the transmission of "form, species and life"; for the study of hybrids shows the error of his opinion that creative force or vital power is

derived exclusively from the male, and proves that both parents must be efficient in determining form or species.

The medical men of Harvey's day held a different opinion, as he tells us in Exercise the thirty-second. Like Aristotle, they held that the embryo arises from "excrement"; but they held, in opposition to his teaching, "that the prime matter of conception is not blood, but the mingled genitures of both sexes." They also held, in opposition to Aristotle, the opinion, which Harvey shares, that the male is no more "the efficient cause of generation" than the female.

"Conception, according to the opinion of medical men, takes place in the following way: during intercourse the male and female dissolve in one voluptuous sensation, and eject their seminal fluids (geniturae) into the cavity of the uterus, where that which each contributes is mingled with that which the other supplies, the mixture having both equally the faculty of action and the force of matter; and according to the predominance of this or that geniture does the progeny turn out male or female. It is further imagined that immediately after the intercourse something of the conception is formed in the uterus."

If the uterus contains a "conception" immediately after a fertile union, in the form of a bloody coagulum, as Aristotle supposes, or in the form of the mingled emissions or genitures of both sexes, as the medical men taught, this ought to be discoverable, and Harvey, a true scientific investigator, set himself to hunt for it without a microscope.

His facilities for making the search, and its results, are best described in his own words. He was the attending physician of the King of England, and he tells us: "It was customary with his Serene Majesty, King Charles, after he had come to man's estate, to take the diversion of hunting almost every week, both for the sake of finding relaxation from grave cares and for his health; the chase was principally the buck and the doe, and no prince in the world had greater herds of deer. This gave me an opportunity of dissecting these animals almost every day during the whole of the season when they were rutting, taking the male and falling with young. I had occasion so often as I desired it to examine and study all their parts, particularly those devoted to the offices of generation."

His studies upon the development of the embryo of the deer are fully described at length in the essay on generation, but only those which relate to the question of conception concern us at present. Here his researches had a very definite result. "Repeated dissections performed in the course of the month of October, both before the rutting season was over and after it had passed, never enabled me to discover any blood or semen or a trace of anything else, either in the body of the uterus or its cornua." Neither the bloody coagulum of Aristotle nor the geniture of the medical men has any existence. The "conception" which should be discoverable in the uterus if their teachings are correct, cannot be found there when a search is made for it, and actual observation shows that their teachings are erroneous and fanciful.

The keepers and huntsmen said "that I was both deceiving myself and had misled the king, and that there must of necessity be something of the conception to be found in the uterus. These men, however, when I got them to bring their own eyes to the inquiry, gave up the point." Harvey tells us that the king fully appreciated the value of the investigation, and in order "that this important question might be the more satisfactorily settled in all time to come," provided means for isolating the does and for proving that there was no error as to the fact of conception; but the physicians were still unconvinced, and "held it among their impossibilities that any conception should ever be formed without the presence of the semen masculinum, or some trace remaining of a fertile intercourse within the cavity of the womb." But the man who had proved the error of their teachings regarding the function of the heart and blood-vessels had little tolerance for their belief in anything which they were unable to demonstrate.

If they had insisted that Harvey's resources were inadequate, that the "conception" for which he sought is too minute to be found by such rough means, we now know they would have been in the right, for even at the present day our knowledge of the essential facts of mammalian conception is, for the most part, a deduction from observations on the eggs of animals which were almost or quite unknown to Harvey, the sea-urchin and ascaris, for example. But his proof of the non-existence, in the uterus of the doe, of anything corresponding to their teachings is conclusive. He did not stop here, however, for he tells us: "In the dog, rabbit and several other animals, I have found nothing in the uterus for several days after intercourse; I therefore regard it as demonstrated that after fertile intercourse among viviparous as well as oviparous animals there are no remains in the uterus either of the semen of the male or of the female emitted in the act; nothing produced by any mixture of these two fluids, as medical writers maintain, nothing of the menstrual blood present as 'matter' in the way Aristotle will have it; in a word, that there is not necessarily even a trace of the conception to be seen immediately after fruitful union of the sexes. It is not true, consequently, that in a prolific connexion there must be any prepared matter in the uterus, which the semen masculinum, acting as a coagulating agent, should congeal, concoct and fashion or bring into a positive generative act."

His study of the generation of birds leads him to the same result. "As the hen does not emit any seminal fluid, and as the seminal fluid of the cock does not reach the uterus of the hen, and as there is no trace of an egg to be found in the uterus immediately after intercourse, it is obvious that it is not engendered" by the mixture of seminal fluid in the way the medical men teach (Exercise the thirty-second). After quoting Aristotle's opinion that the chick is formed out of menstrual blood coagulated by the influence of the cock, he says (Exercise the twenty-first): "The business in the generation of an egg is very different from this; for neither does the semen or rather the 'genitura' proceeding from the male in the act of intercourse, enter the uterus in any way, nor has the hen after she conceives any particle of excrementitious matter, even of the purest kind, or any blood in her uterus which might be fashioned or perfected by the discharge of the male. Neither are the parts of the egg produced by any kind of coagulation; neither is there anything like curdled milk to be discovered in the uterus. The cock, I say, contributes neither form nor

matter to the egg, but that only by which it becomes fertile and fit to engender a chick. And this faculty the cock confers by his semen (genitura) emitted in the act of intercourse, not only on the egg which is already begun, but on the uterus and ovary and even on the body of the fowl herself, in such wise that eggs which have yet to be produced, eggs, none of the matter of which yet exists either in the ovary or in any other part of the body, are thence produced possessed of fecundity." "Inflammable material is not set on fire by the contact of flame more quickly than is the hen made pregnant by intercourse with the cock" (315).

Careful observation on the fowl, the deer, the dog, the rabbit, and on many other animals, proves that none of them are generated out of excrement or decomposing matter. There is no basis in nature for Aristotle's opinion or that of the medical men, and all these teachings break down when brought to the test of actual observation. It is no small thing to prove the error of the belief, which had been current for two thousand years, and is even now embodied, through a quotation from St. Paul, in our burial service, that all forms of reproduction find their type in generation from dead putrescent matter. This Harvey accomplished by methods which are rigorously scientific; and with this accurate but very imperfect knowledge he boldly faced and tried to answer the question, what is it which the cock contributes in virtue of which the egg "becomes fertile and fit to engender a chick"?

He undertakes "to seek the truth regarding the following difficult questions: Which and what principle is it whence motion and generation proceed? By what virtue does the semen act? What is it that renders the semen itself fruitful? Whether is that which in the egg is cause, artificer and principle of generation and of all the vital and vegetative operations—conservation, nutrition, growth—innate or superadded? and whether does it inhere primarily, of itself, and as a kind of nature, or intervene by accident, as the physician in curing disease? Whether is that which transfers the egg into a pullet inherent or acquired, or is it already conceived in the ovary, and does it nourish, augment, and perfect the egg there? What is it besides that preserves the egg sweet after it is laid? What is it that renders an egg fruitful?" (274).

"In truth, there is no proposition more magnificent to investigate or more useful to ascertain than this: How are all things formed by an 'univocal agent'? How does the like ever generate the like? . . . Why may not the thoughts, opinions, and manners now prevalent, many years hence return again, after an intermediate period of neglect?" (582).

As we find the embryologists of the present day vexing themselves over the question, "Whether is that which transfers the egg into a pullet inherent or acquired?" we need not wonder if Harvey's success in the investigation of this magnificent proposition seems small to us. At least we must follow him in order to understand his dictum.

As a starting-point this much seems to be certain. "The egg, even when contained in the ovary, does not live by the vitality of the mother, but is like the youth who comes of age, made independent even from its first appearance; as the acorn taken from the oak, and the seeds of plants in general are no longer to be considered parts of the tree or herb that supported them, but things made in their own right, and which already enjoy life in virtue of a proper and inherent vegetative power" (275).

Furthermore, "although some animals . . . are produced from females alone" (286); "it is manifest that a fruitful [hen's] egg cannot be produced without the concurrence of a cock and a hen; without the hen no egg can be formed; without the cock it cannot become fruitful. But this view is opposed to the opinion of those who derive the origin of animals from the slime of the ground" (284).

"The egg is the terminus from which all fowls, male and female, have sprung, and to which all their lives tend—it is the result which nature has proposed to herself in their being" (271).

"And this is the round that makes the race of the common fowl eternal; now pullet, now egg, the series is continued in perpetuity; from frail and perishing individuals an immortal species is engendered" (285).

"We cannot conceive an egg without the concurrence of a male and female fowl any more than we can conceive fruit to be produced without a tree. We therefore see individuals, males as well as females, existing for the sake of preparing eggs, that the species may be perennial though their authors pass away. And it is indeed obvious that the parents are no longer youthful or beautiful, or lusty, and fitted to enjoy life, than while they possess the power of producing and fecundating eggs, and by the medium of these, of engendering their like. But when they have accomplished this grand purpose of nature, they have already attained to the height, the ἀκμή of their being; the final end of their existence has been accomplished; after this, effete and useless, they begin to wither, and, as if cast off and forsaken of nature and the Deity, they grow old, and, a-weary of their lives, they hasten to the end. How different the males when they make themselves up for intercourse, and swelling with desire are excited by venereal impulse! It is surprising to see with what passion they are inflamed, and then how trimly they are feathered, how vainglorious they show themselves, how proud of their strength, and how pugnacious they prove. But the grand business of life accomplished, how suddenly, and with failing strength and pristine fervor quenched, do they take in their swelling sails, and from late pugnacity, grow timid and desponding. Even during the season of jocund masking in Venus's domains, male animals in general are depressed by intercourse, and become submissive and pusillanimous, as if reminded that in imparting life to others they were contributing to their own destruction. The cock alone, replete with spirit and fecundity, still shows himself alert and gay, clapping his wings and crowing triumphantly he sings the nuptial song at each of his espousals; yet even he after some length of time in Venus's service, begins to fail; like the veteran soldier, he by and by craves discharge from active duty, and the hen, too, like the tree that is past bearing, becomes effete, and is finally exhausted."

Having come to the end of his means of observation, Harvey turns to reflection, the second resource of the man of science, to see how this may help him to discover how "from frail and perishing individuals an immortal species is engen-

dered." As his studies seemed to prove that the contagion which remains in the female "after intercourse, as the efficient of the future offspring, is not of the nature of any corporeal substance," he was unable to escape the admission that it is "incorporeal." Thus driven to the wall, if he had taken refuge in "soul" or "spirit," no one could greatly blame him, for spiritual agents had been the resource of philosophers for many ages before his time. He was a true soldier of science, however, seeing as clearly as we do that this venerable formula can do nothing to help us, and preferring outspoken ignorance to this antiquated and threadbare cloak for intellectual poverty.

"If on further inquiry it should appear that it [the efficient] is neither spirit nor demon, nor soul, nor any part of a soul, as I believe can be proved by various arguments and experiments, what remains, since I am unable myself to conjecture anything beside . . . but to confess myself at a standstill?"

What does the modern man of science in such a case? Does he not search through the whole province of knowledge to see if perchance he may find some other natural phenomenon which bears some resemblance to the subject of his studies? Harvey says he knows well "that some censorious persons will laugh at this . . . Yet this that I do is the practice of philosophers, who when they cannot clearly comprehend how a thing really is brought to pass, devise some mode for it in accordance with the other works of nature, and as near as possible to what is true."

"Since, then, nothing can be apprehended by the senses in the uterus after coition, and since it is necessary that there be something to render the female fruitful, and as this is probably not material, it remains for us to take refuge in a mere conception."

Men of science in all ages, from Aristotle to Tyndall, have believed in the virtues of the provisional hypothesis; and, armed by eminent authority, Harvey undertakes, by comparing a "mere conception" with other things in nature, to frame a provisional hypothesis of generation; but natural science seems to be an uncongenial soil for the nurture of such attempts, and if time has shown that Harvey's hypothesis has little value, he errs in good company, and he also takes pains to say he does not wish it "to be taken as if I thought it a voice from an oracle," although he does hope it may "stir up the intellects of the studious to search more deeply into so obscure a subject."

Starting with the belief that "the semen of the male does not so much as reach the cavity of the uterus . . . and that it carries with it a fecundating power by a kind of contagious property" from which the female "seems to receive influence and to become fecundated without the co-operation of any sensible corporeal agent, in the same way as iron touched by the magnet is endowed with its powers and can attract other iron to itself," he holds that "when this virtue is once received the woman exercises a plastic power and produces a being after her own image."

"Yet it is a matter of wonder where this faculty abides after intercourse is completed. . . . To what is the active power of the male committed? . . . Does the woman conceive in the womb as we see by the eye and think by the brain?" "Since there are no manifest signs of conception before the uterus begins to relax . . . and since the substance of the uterus, when ready to conceive, is very like the structure of the brain, why should we not suppose that the function of both is similar, and that there is excited by coitus within the uterus a something identical with or at least analogous to an imagination or a desire in the brain, whence comes the generation or procreation of the ovum?" "For the functions of both are termed conceptions, and both, although the primary source of every action throughout the body, are immaterial, the one of natural or organic, the other of animal action . . . Just as a desire arises as a conception of the brain, and this conception springs from some external object of desire, so also from the male, as being the more perfect animal, and as it were the most natural object of desire, does the natural (organic) conception arise in the uterus, even as the animal conception does in the brain. From this desire, or conception, it results that the female produces an offspring like the father. For just as we, from the conception of the 'form' or 'idea' in the brain, fashion in our works a form resembling it, so in like manner the 'idea' or 'form' of the father, existing in the uterus, generates an offspring like himself with the help of the formative faculty.

"Whoever has pondered with himself how the brain of the artist, or rather the artist by means of his brain, pictures to the life things which are not present in him, but which he has once seen; also in what manner birds immured in cages recall to mind the spring, and chant exactly the songs they had learned the preceding summer, although meanwhile they had never practiced them; again, and this is more strange, how the bird artistically builds its nest, the copy of which it had never seen, and this not from memory or habit, but by means of an imaginative faculty, and how the spider weaves its web, without either copy or brain, solely by the help of this imaginative power; whoever, I say, ponders these things, will not, I think, regard it as absurd or monstrous, that the woman should be impregnated by the conception of a general immaterial 'idea' and become the artificer of generation."

"For my own part then, when I see nothing left in the uterus after intercourse, to which I can ascribe the principle of generation, any more than there is in the brain anything discoverable after sensation and experience, which are the prime sources of art, and when I find the structure of both alike, I have devised this fable."

Whatever the value of this hypothesis may be, it serves well to emphasize the fact that Harvey's opinions on generation have nothing in common with the modern discovery of the physical continuity of living matter, and it shows that his teaching that all animals come from eggs cannot possibly mean what the words now mean.

He believes the uterus conceives an animal in the same way that the brain conceives an idea; and he also tells us that he agrees with Fabricius that "the egg [of the hen] is in a certain sense an exposed uterus" (290).

Furthermore, "the hen is not the efficient cause of a perfect egg, but that she is made so in virtue of an authority, if I may use the word, or power required of the cock. For the egg, unless prolific, can with no kind of propriety be accounted perfect; it only obtains perfection from the male, or rather

from the female, as it were upon precept from the male, as if the hen received the art and reason, the form and laws of the future embryo from his addresses" (290). So much for the generation of the fowl. In Exercise the sixty-ninth he describes the embryo of the doe at about seven weeks, and the human embryo about the second month after conception, but, following Aristotle, he regards these embryos in their membranes, not as embryos, but as eggs without shells. "In the way above indicated do the hind and doe, affected by a kind of contagion, finally conceive and produce primordia, of the nature of eggs, or the seeds of plants, or the fruit of trees, although for a whole month and more they had exhibited nothing in the uterus."

In this sense, then, he holds that viviparous animals are generated from eggs. He therefore maintains (as contrasted with Fabricius, who held that the greater number of animals are produced from ova) "that all animals, even the vivipara, and man himself not excepted, are produced from ova; that the first conception, from which the foetus proceeds in all, is an ovum of one description or another, as well as the seed of all kinds of plants. Empedocles therefore spoke not improperly of the 'egg-bearing race of trees.' The history of the egg is therefore of the widest scope, as it illustrates generation of every description. . . . Fabricius has these additional words: "The foetus of animals is engendered in one case from an ovum, in another from the seminal fluid, in a third from putrefaction; whence some creatures are oviparous, some viviparous, and yet others born of putrefaction or by the spontaneous act of nature, automatically."

"Such a division as this, however, does not satisfy me, inasmuch as all animals whatsoever may be said in a certain sense to spring from ova, and in another sense from seminal fluid, and they are entitled oviparous, viviparous or vermiparous rather in respect of their mode of bringing forth than of their first formation."

We see then that, unfamiliar as his words often seem, and while he holds that the organizing influence which produces the chick from the egg is a "divine mystery," we owe to Harvey the demonstration and clear formulation of the following truths:

There is no basis for the venerable doctrine that the higher animals are generated from excrement.

The hen's egg, even before it leaves the ovary, is an independent organism, which enjoys life by its own right, and perfects itself by nutrition.

The embryo assimilates homogeneous food, and by means of an inherent organizing power converts it into the structure of the living animal Nutrition, growth and development go on together, and the embryo arises by epigenesis or differentiation.

Many animals which have been held to arise from putrescent slime actually come from microscopic eggs.

"Animals are entitled viviparous or oviparous or vermiparous rather in respect of their mode of bringing forth than of their first formation."

LONG, THE DISCOVERER OF ANÆSTHESIA.

A PRESENTATION OF HIS ORIGINAL DOCUMENTS.

By Hugh H. Young, A. M., M. D., *Assistant Resident Surgeon, Johns Hopkins Hospital, Baltimore.*

[*Being in substance a paper read before the Johns Hopkins Hospital Historical Society, November* 8, 1896, *with additions.*]

It was my good fortune last summer to meet Mrs. Fanny Long Taylor, whose father, Dr. Crawford W. Long, is thought by many to be the original discoverer of anæsthesia.

As she put me in possession of her father's papers, I thought they might prove sufficiently interesting to warrant their presentation to this Society.

During the famous ether controversy which was waged in the forties and fifties the work of Long received little attention. A modest, retiring man, who abhorred public strife and controversy, too honorable to wish pecuniary reward for his discovery, it is not strange that he made no effort to get the reward from Congress, but preferred to let the justice of his claim be judged by an unbiased posterity.

Crawford W. Long was born in Danielsville, Ga., on the first of November, 1815. His grandfather was Capt. Samuel Long of Pennsylvania, who made a brilliant record in the Revolutionary war and was one of Lafayette's captains at Yorktown. Soon after the close of that eventful struggle he left his native State and took his family to Georgia, where they settled along with a large colony of Pennsylvanians.

His son, James Long, received every educational advantage there obtainable, and inheriting his father's executive ability, became one of the prominent men of his State. Although engaged in mercantile pursuits he was a hard student of the law, and so well versed in the principles of jurisprudence that he was often consulted by judges in difficult cases.

He represented his people in the Senate for a number of years and was the intimate friend, adviser and confidant of Wm. H. Crawford, Georgia's greatest statesman, at different times Secretary of State, minister to France and candidate for the Vice-Presidency. As an evidence of attachment he gave his eldest son the name of Crawford. Coming from such ancestry it is not wonderful that young Crawford early showed promise of rare ability. As a boy he was studious and mature beyond his years, and entered Franklin College at so early an age that he was known as "baby." Notwithstanding this fact he graduated as Master of Arts second in his class at the age of nineteen. Alexander H. Stephens was his room-mate, and so much older that he was dubbed "daddy" by the college boys. Though pursuing different paths, Stephens and Long kept up an intimate friendship all their lives. After studying under a preceptor for one year

Long graduated at the University of Pennsylvania in 1839. While there he was recognized as a man of marked ability and fond of experimental work.

After graduation he spent one year in a New York hospital, where, it is said, he made such a reputation for himself as a surgeon that he was urged to apply for the position of surgeon in the U. S. Navy. Obedient to his father's wishes, however, he returned to practice in his native State and located in Jefferson, Jackson Co., Georgia, in 1841, at that time a small country town, far removed from any railroad, in the midst of a farming community whose only factory was the cotton gin.

Here Dr. Long soon acquired an extensive and lucrative practice, and although young was noted for his quiet, dignified bearing, which endeared him to all. His office was a favorite meeting-place for the young men of the town, who would often gather there to pass a merry evening together.

About this time it became fashionable to inhale laughing-gas for its exhilarating effects. Itinerant lecturers on chemistry would conclude an evening's entertainment with a nitrous-oxide party in which the participants would get gloriously drunk from its inspiration.

The practice spread throughout the country, and at Jefferson during the early part of the winter of '41 a coterie of young friends begged Dr. Long to let them indulge in the far-famed luxury.

Dr. Long replied that he had no means of preparing nitrous-oxide gas, but that sulphuric ether would produce similar exhilaration. The company being anxious to try it, the ether was produced, all inhaled of it and soon became hilarious.

The young men were delighted and hastened to tell their friends of Dr. Long's wonderful drug, and thus the inhalation of ether for sport soon became very popular over that section of Georgia, and almost every party ended up with an "ether frolic," as it was called.

During January, 1842, the ether frolics at Dr. Long's office became very frequent and were well attended, and some of the young men probably became pretty thoroughly intoxicated, as Dr. Long discovered that he and others would afterward have considerable bruises about their persons of which they had no knowledge.

Being a thoughtful man, *he at once remarked that ether must have the power of rendering one insensible to pain, and therefore available for preventing pain in surgical operations.*

This was January, 1842, and Dr. Long at once determined to prove his discovery on the first surgical case he should have. That opportunity came on March 30th, when Long administered ether to Mr. James M. Venable till completely anæsthetized, and then excised a small cystic tumor from the back of his neck. Imagine the surprise of the patient when on regaining consciousness he was told that the operation was over, and his amazement when he saw the tumor in the hands of the surgeon and he had not felt a scratch. This was four and one-half years before Morton's earliest claims.

An original paper read by Dr. Long before the Georgia State Medical Society in 1852, describing these events in his own words, is appended to this paper—see Appendix I.

It is worthy of note that two and a half years later Wells of Hartford discovered the anæsthetic powers of nitrous oxide under very similar circumstances. He attended an entertainment given by a popular lecturer on chemistry, inhaled nitrous oxide for its exhilaration, and saw a man under its influence injure his ankle severely without being conscious of it. From this he concluded that nitrous oxide was capable of producing anæsthesia and proved it in extracting a tooth.

Morton, on the other hand, got his suggestion as to the anæsthetic power of ether from Jackson, who, after using ether to relieve the pain and dyspnœa following the accidental inhalation of chlorine gas—an antidote well known in chemistry then—inferred that it might be useful as an anæsthetic. Jackson did not test the correctness of this observation, which he claims to have made about the same time that Long etherized his first case, but left it for Morton to prove practically four and a half years later.

Dr. Long reports the first five cases in which he used ether, being desirous only of establishing priority of use. An examination of the letters and certificates before me, however, shows that he must have operated on at least eight cases before Morton's "discovery." This number seems small, but is not so astonishing when we remember that the country was sparsely settled, that Jefferson was a mere village, and Long had just entered practice.

I will now read in substantiation of these statements a copy of the original account in Dr. Long's journal against Mr. Venable for medical services rendered, certified to by a clerk of the Superior Court.

"James Venable

To Dr. C. W. Long, Dr.

1842.	cts.
January 28th, sulphuric ether,	.25
March 30th, sulphuric ether and exsecting tumor,	2.00
May 13th, sul. ether,	.25
June 6th, exsecting tumor,	2.00

Georgia, Jackson County. } I, P. F. Hinton, clerk of the superior court of said county, do certify that the above account is a correct copy of an original entry made in his book for medical services for the year 1842.

Given under my name and seal of office this 27th of March, 1854.

[Seal] (Signed) P. F. HINTON, *Clerk S.*"

The following paper relative to the fashion of inhaling ether for its exhilarating effects is interesting as showing how the custom in Georgia started with Dr. Long:

"I certify that on the first of January, 1842, I resided in Jefferson, Jackson Co., Georgia, and that about that time myself with several other young men were in the habit of meeting at Doct. C. W. Long's *shop*, and other rooms in the village, and inhaling ether which *he administered to us*.

On the 20th of January of the same year I removed to Athens, where I introduced the inhalation of ether. I and several of my young associates frequently assembled ourselves together and took it for the excitement it produced. After that I know it became very common to inhale ether in Athens, and that it was frequently taken in the college campus and on the street.

(Signed) R. H. GOODMAN,

August 4th, 1849. Athens, Georgia."

When we see daily the dreadful distaste patients who have been etherized have for ether, it seems strange that any one could become fond of its use.

I will now read an account of the first operation as given by Mr. James M. Venable:

"I, James M. Venable, of the county of Cobb and State of Georgia, on oath depose and say, that in the year 1842 I resided at my mother's in Jackson County, about two miles from the village of Jefferson, and attended the village academy that year.

In the early part of the year the young men of Jefferson and the country adjoining were in the habit of inhaling ether for its exhilarating powers, and I inhaled it frequently for that purpose, and was very fond of its use.

While attending the academy I was frequently in the office of Dr. C. W. Long, and having two tumors on the back of my neck, I several times spoke to him about the propriety of cutting them out, but postponed the operation from time to time. On one occasion we had some conversation about the probability that the tumors might be cut out while I was under the influence of s. ether, without my experiencing pain, and he proposed operating on me while under its influence.

I agreed to have one tumor cut out and had the operation performed that evening after school was dismissed. This was in the early part of the spring of 1842.

I commenced inhaling the ether before the operation was commenced and continued it until the operation was over. I did not feel the slightest pain from the operation and could not believe the tumor was removed until it was shown to me.

A month or two after this time Dr. C. W. Long cut out the other tumor, situated on the same side of my neck. In this operation I did not feel the least pain until the last cut was made, when I felt a little pain. In this operation I stopped inhaling the ether before the operation was finished.

I inhaled the ether, in both cases, from a towel, which was the common method of taking it.

Georgia,
Cobb Co.,
July 23rd, 1849. } Sworn to before me.

(Signed) JAMES M. VENABLE.

ALFRED MANES, J. P."

This operation was done in the presence of four witnesses, Jas. E. Hayes, A. T. Thurmond, W. H. Thurmond, principal of the academy, and Edmund S. Rawls, the last of whom testifies as follows:

"Georgia, } I, Edmund S. Rawls, of Rome, Floyd Co., Ga., on
Clarke Co. } oath depose and say that . . . on one occasion during that year (1842) I was present with James M. Venable in the office of Dr. C. W. Long in Jefferson, Jackson Co., Ga., and witnessed Dr. C. W. Long cut out a tumor from the side of neck of J. M. Venable while said Venable was fully under the effects of the vapor of s. ether inhaled from a towel, and without his exhibiting the least symptoms of suffering pain from the operation. J. M. Venable was so unconscious of the operation having been performed that he would not believe the tumor was removed until it was shown him. (Signed) E. S. RAWLS.

Sworn to and subscribed before me this 2nd November, 1853.

E. L. NEWTON, *J. J. C.*"

The patient continued to inhale ether until the operation was over, was entirely unconscious of its performance, and felt no pain. Surely this was complete anæsthesia. This fact has been denied by Dr. Wm. J. Morton, son of the Boston discoverer, who says it was no more than mere exhilaration.

It has been stated that Long kept his discovery secret and that he therefore deserved no credit for it. I present certificates from Drs. Laperriere and Carlton, which show that his work was well known to citizens of the town of Jefferson and neighboring cities, particularly Athens, which was then the centre of learning and culture in Georgia; that it was considered a remarkable discovery by the populace, and that the prominent physicians knew of it and realized its importance. See Appendix II.

Dr. Carlton was then a student under Dr. Moore. One year later (1844) he used ether in extracting a tooth. This was probably the first use of ether in extracting teeth. Dr. James Camak, another student of Moore, was present and assisted in the same operation and corroborates Carlton's statements.

Can Dr. Long be blamed for not publishing at once a report of his first case or two when they were well known to the physicians of that part of the State? He had no Massachusetts General Hospital at his back, and he knew that such startling claims coming from one so young (he had not been practicing one year) would be severely criticised. It is but natural that he should be satisfied for the time being with the local and State publicity which was given to his great discovery, and waited until he could make a comprehensive report embracing all kinds of cases, such as every careful investigator does to-day.

But he kept on with his work, operating on two more cases under ether in 1842, and about three more during the next year, for most of which I find sworn certificates.

I have a letter from his first student, Dr. J. F. Groves, which is of particular interest as giving an insight into the character of Dr. Long and his work at that time. It is quite lengthy and I omit portions of it. The letter is written to Mrs. Taylor, Dr. Long's eldest daughter. See Appendix III.

This letter shows conclusively that Dr. Long was thoroughly convinced of the anæsthetic powers of ether, but was anxious to put it to a severe test in capital surgery. He withheld his cases of minor surgery because he wished to determine accurately the limitations and possibilities of ether.

In his paper Dr. Long does not give the details of the etherization with the minuteness we should desire. For instance, he does not say who gave the ether, the patient or himself, and he does not explain whether the patient was entirely unconscious or not, but simply remarks that he suffered no pain and did not know the tumor had been removed.

These omissions of Dr. Long led Dr. Wm. J. Morton, of New York, to write a forty-eight-page article in the Virginia Medical Monthly, March, 1880, in which by dexterously quibbling with Long's innocent statements he arrives at the conclusion by a skilful process of deduction, that Long never did anything. I will read parts of this article:

"THE INVENTION OF ANÆSTHETIC INHALATION.

* * * But we will proceed slowly. We must know who is giving the ether. Of the first operation Venable deposes under oath, 'I commenced inhaling the vapor before the operation was commenced and continued it until the operation was over.'

Thus Venable kept his eye on the whole affair, knew just what was going on, otherwise how could he know and swear to it that he continued the inhalation until the operation was over. Surely *Venable administered the ether to himself and remained conscious all the time.*"

This is erroneous, for according to good legal authority

such a statement as Venable's could be made from a knowledge based on satisfactory hearsay evidence and would be accepted in law.

But he goes on: "But now comes the damnatory point of this second experiment. *The patient felt pain.* This both Long and Venable confess. Here then is positive failure; Dr. Long's anæsthetic state was nothing more nor less than the fleeting peripheral numbness often associated with the first or exhilarating stage of the complete anæsthesia of to-day. But why did Dr. Long not take the towel into his own hand and force the ether? Why not make his patient insensible to pain if he knew this could be done?

"Knowing what medicine knows to-day, how wonderful this halting of Long—this pause at a most critical moment—on the very threshold of discovery! So nicely balanced is the situation that it almost seems as if he would topple over into discovery; but he falls the other way.

"It seems almost inexplicable that he did not seize the towel, force the anæsthesia to the stage of stupor, perform the operation, and proclaim the discovery to the world,"—

and we might add, patent it at once!

It is a fact well known to all surgeons that a patient may be entirely unconscious and still retain sensibility to pain. This is evidenced by the flinching before the knife when in this state. He may give evidence of feeling pain, but have no recollection of it afterward. All witnesses agree that Venable gave no evidence of feeling pain in the first operation. He must have been pretty thoroughly anæsthetized. The second operation was merely a test case to determine the length of the anæsthetic state, and although the ether was discontinued from the beginning of the operation (which Long says was tolerably difficult on account of adhesions), still the anæsthetic state lasted until the last cut was made. We would consider such a patient pretty thoroughly anæsthetized in Baltimore.

DR. CRAWFORD W. LONG.

I have recently tested a number of patients who were being anæsthetized and found invariably that consciousness was lost before sensibility to pain—as evinced by movement of a member when pricked with a pin.

It was therefore practically certain that no person could administer ether to himself, an act requiring consciousness—and become sufficiently anæsthetized for even very small operations; but in order to settle the question definitely I determined to administer ether to myself, using the same methods as were employed by Long in 1842.

Accordingly, under the direction of Dr. F. R. Hagner of Washington, I placed a folded towel over my face and poured ether upon it from time to time at his bidding, as long as I was conscious. During this time Dr. Hagner pricked me with pins up to the last application that I remember. Toward the last the pin pricks did not produce the usual sensation of pain, but a peculiar disagreeable clanging sound in the ear.

Dr. Hagner thus briefly describes the experiment: "Dr. Young poured ether on the towel when so instructed by me. After a number of such additions the movements of his arm became very unsteady and he would spill some of the ether on his neck. Soon after the last application, made at my request, I pricked him with a pin and he moved his leg. I then instructed him several times to add more, but he made no attempt to do so and seemed unconscious. I then pricked him with pins several times, but received no response for about a minute, when he again responded to a prick of pin and suddenly became conscious. Complete consciousness returned almost immediately.

"I feel certain that he lost consciousness before sensibility to pain, and that if I had attempted to do the smallest surgical operation he would have been conscious of pain before its completion."

It is therefore certain that Venable would have been conscious long before the small tumor was removed and would have suffered considerable pain, if he had conducted his own etherization. Nor is it probable that Long intended to convey that idea. We might with propriety say to-day that our patients "inhale ether until anæsthetized," if we did not care to specify the details of etherization. But in a document quoted above R. H. Goodman says, in speaking of the ether frolics, "We were in the habit of meeting at Dr. Long's shop and *inhaling ether which he administered to us.*" It seems probable therefore that some one administered ether to the persons on whom he operated, too.

Desirous of settling these trivial points on which Morton would have Long deprived of every honor, I addressed a letter a few days ago to Long's first student, Dr. J. F. Groves, whose document I have quoted above. I saw that he would now be about 75 years of age and waited for an answer with considerable anxiety. To my surprise and delight I received a letter from him yesterday which supplies all of Dr. Long's omissions. Dr. Groves did not see the first operations, as he did not enter Dr. Long's office till 1844, but soon after his entrance he assisted in the operation on the negro boy in which two fingers were amputated, early in 1845, and at my request he describes this etherization in detail.

As this case occurred a year and a half before Morton's discovery, it is equally good as the first for my purposes. It is as follows:

A Recent Letter from Long's First Student.

"Cohutta, Ga., Jan. 15th, 1897.

Dr. Hugh H. Young,

Dear Sir: * * * The patient was placed in a recumbent position, on a bed, with hand to be operated on to the front for convenience to the surgeon. Dr. Long poured ether on a towel and held it to the patient's nose and mouth, too, to get the benefit of inhalation from both sources. Dr. Long determined when the patient was sufficiently etherized to begin the operation by pinching or pricking him with a pin. Believing that no harm would come of its use for a reasonable length of time he profoundly anæsthetized the patient, then gave me the towel and I kept up the influence by holding it still to the patient's nose. The patient was entirely unconscious—no struggling—patient passive in the hands of operator. After a lapse of fifty years you would hardly suppose that a man could remember every minute detail, but I have clearly in mind all the facts I have given you.

Your ob't servant,

(Signed) J. F. Groves, M. D."

Long then administered ether as it is done to-day. He did not pause at the threshold of discovery or topple the other way, but kept right ahead and by careful observation, experimentation and reflection discovered that ether was a safe, sure and complete anæsthetic.

Nor did his patients etherize themselves and at the same time superintend the operation, as Dr. Morton would have us believe.

As quoted above, Morton grasps eagerly at the acknowledgment of pain at the end of the second operation, where the ether had been discontinued, and from tnat isolated instance characterizes all of Dr. Long's cases as failures. But in his eagerness to annihilate Dr. Long he seems to have overlooked the fact that in the first report of anæsthesia with Morton's "Letheon" at the Massachusetts General in 1846, Dr. Bigelow* says, in speaking of the first operation, "During the operation the patient muttered as in a semi-unconscious state, and afterwards stated that the pain was considerable," and in the second, "The operation lasted four or five minutes, during which time the patient betrayed occasional marks of uneasiness." But these certainly were not failures, although the anæsthesia was not as complete as in Dr. Long's previous cases.

In his paper Long speaks of "those high in authority who were advocating the mesmeric state as adequate to prevent pain in surgical operations." Contemporary medical literature furnishes ample verification of these statements, according to Dr. Grandy,† who says:

"The journals were full of discussions upon the phenomena of mesmerism, animal magnetism, etc., and wonderful reports were coming from European hospitals of operations done without pain during the 'magnetic sleep.'

"Jules Cloquet had excised a cancerous breast with the axillary glands and the patient showed no sign of pain. Topham of London, in 1843, had amputated a thigh, and Dr. Dugas of Augusta, Georgia, in 1845, twice extirpated the mamma of a female under the mesmeric sleep.

"We can imagine what effect these cases had on the mind of Long. Need we wonder therefore that he was the more particular in his experiments on etherization. Such were the reasons for his silence, and while the sequel was unfortunate, his course was cautious and commendable."

While thus waiting, his opportunity was lost. A second discovery was made in Boston in 1846, was published immediately, and anæsthesia became the property of the world.

Friends of the other "discoverers" have often stated that as Long made no publication of his work he deserved no credit for it. To this Dr. J. Marion Sims responds very forcibly as follows:*

"Now upon this point Long, Wells, Morton and Jackson stand individually upon the same level.

Long exhibited to medical men and to the community his operations under ether in 1842. Wells exhibited to medical men and to the community his operations of the extraction of teeth under nitrous oxide gas in 1844. Morton exhibited to medical men and the community the use of his secret remedy "Letheon," 1846, as an anæsthetic. But Morton was fortunate in showing his patent remedy to the great surgeons of Boston, and it was not Morton, but it was Warren and Hayward and Bigelow who performed the operations to which the world owes the immediate and universal use of anæsthesia in surgery. If Morton could have had his way he would have deodorized the ether and kept it secret from the world. Neither Wells nor Morton nor Jackson ever published a word on the subject till it burst forth in a blaze from the labors of the hospital surgeons already named. When Warren and Hayward and Bigelow proved the real greatness of the discovery [and published it broadcast], then it was that Wells, Morton and Jackson began the war of pamphlets, and not till then did either of them publish in any scientific journal a line about anæsthesia."

The Famous Ether Controversy.

In 1849 Morton petitioned Congress for a reward for his discovery. He was at once opposed by Jackson and the friends of Wells, who was then dead. The celebrated ether controversy, thus begun, occupied the attention of Congress for many years, and was characterized by the greatest animosity between these former bosom friends and companions.

For five years Long refused to take part in the conflict, but finally in 1854, persuaded by his friends that in that way alone could he obtain recognition of his claims, he wrote to Senator Dawson giving an account of his work. It seems that Dawson was a friend of Jackson, for he wrote to him of this new claimant and requested him to investigate his case. This Jackson did, calling upon Long at his home in Athens on March 8th, 1854.

The Interview between Long and Jackson.

At this interview Hon. C. W. Andrews, a prominent justice,

* Boston Medical and Surgical Journal, Nov. 1846.

† Va. Med. Monthly, Oct. 1893.

*Va. Med. Monthly, May, 1877.

was present, and certifies that after satisfying himself of the genuineness of the claims, Jackson proposed to Long to lay their claims conjointly before Congress—he, Jackson, to claim the discovery, and Long to claim the first practical use,—his object evidently being to get ahead of Morton.

This proposition Dr. Long rejected, being satisfied that he was entitled to both. In a letter to Hon. D. L. Swain, ex-governor of North Carolina, which I have here, he says in regard to this transaction: "The only ground Dr. Jackson urged for his right to the discovery was that while suffering with pain and dyspnœa, in February, 1842, from breathing chlorine gas, he inhaled ether and found that while under its influence he was free from pain. He does not claim that he suggested its use to prevent pain in surgical operations until more than one year after my first operation was performed. I cannot give the exact date when I was first led to believe that ether would prevent pain in surgical operations, but I know it was as early as February, 1842."

Now in 1839 Pereira, in his "Elements of Materia Medica," states: "The vapor of ether is inhaled to relieve the effects caused by accidental inhalation of chlorine gas. If the air be too strongly impregnated with ether, stupefaction ensues."

So there was very little new in Dr. Jackson's "discovery," and a mere untried inference hardly deserves the title of discovery.

Dr. Jackson finally acknowledged the justice of Dr. Long's claims and wrote to Senator Dawson to that effect.

On April 15th, 1854, the appropriation bill was up before the Senate for its final reading. The friends of Wells and Morton, relying on the volumes of manuscript they had presented, were confidently awaiting the result, when Senator Dawson arose and said that he had a letter from Dr. Jackson which acknowledged that a Dr. Long in Georgia had undoubtedly used ether before any of the claimants for the appropriation.

Coming as it did from so prominent a contestant, this announcement fell like a thunderbolt on the rival claimants, and from that time they seem to have lost all hope of gaining the reward and passively allowed the bill to die.

Desirous only of preventing another from being recognized by Congress as the discoverer, and not wishing any pecuniary reward himself, Long never pushed the matter farther, and his documents of proof were never even brought up before Congress.

I have here an interesting memento of that conference between the two discoverers, in a card on which Jackson has written a note to Long. On one side it reads:

"For Dr. C. W. Long,
of Athens, Ga.
C. T. Jackson,
New York Hotel.
(over)"

and on the reverse:

"Telegraph from J. L. Hayes, Washington.
'Assignee struck out by request of Mr. Everett.'
Bill probably will come up in House July next."

Edward Everett was then senator from Massachusetts. In the transactions of the Senate, April 19th, 1854, I find the following explanation of the bill by Senator Walker:

"The bill as amended recites that a discovery of anæsthesia has been made—that it is believed the discovery was made by some one of the following persons, W. T. G. Morton, Chas. T. Jackson and Horace Wells, but it does not appear to the satisfaction of Congress which of those parties was the original, true and first discoverer thereof. It proposes to appropriate $100,000.00 as a recompense for the real discoverer. In order to determine this it shall be the duty of the district attorney of the United States for the Northern District of New York, to file in the circuit court of the United States for that district a bill of interpleader wherein reciting the act or its substance, the Secretary of the Treasury shall be complainant, and W. T. G. Morton, Chas. T. Jackson and the personal representatives of Horace Wells or any other person who may make application to the court for that purpose shall be defendants. The issue is to be which of the parties named was the original, true and first discoverer of anæsthesia, and the court is to decide which one that is and direct that the sum of $100,000 be paid over to him."

At the instance of Senator Dawson Dr. Long's name was also inserted in the bill.

I have carefully searched the Congressional Records and find that this bill never came up before the House for final passage, and consequently never reached the district court of New York. It seems to have been abandoned.

Several years later Dr. Jackson wrote an article in the Boston Medical and Surgical Journal giving Long the credit for the first use of ether in surgery. I have here Dr. Long's copy of that journal. The communication is so important that I will read most of it:

"The Boston Medical and Surgical Journal.
Boston, Thursday, Apr. 11, 1861.

FIRST PRACTICAL USE OF ETHER IN SURGICAL OPERATIONS.

Messrs. Editors:—At the request of the Hon. Mr. Dawson, U. S. Senator from Georgia, on March 8th, 1854, I called upon Dr. C. W. Long, of Athens, Georgia, while on my way to the Dahlonega gold mines, and examined Dr. Long's evidence, on which his claims to the first practical operations with ether in surgery were founded, and wrote, as requested, to Mr. Dawson, who was then in the U. S. Senate, all I learned on the subject. From the documents shown me by Dr. Long, it appears that he employed sulphuric ether as an anæsthetic agent:

1st, March 30th, 1842, when he extirpated a small glandular tumor from the neck of James M. Venable, a boy in Jefferson, Georgia, now dead.

2nd, July 3rd, 1842, in the amputation of the toe of a negro boy belonging to Mrs. Hemphill, of Jackson, Ga.

3rd, Sep. 9th, 1843, in extirpation of a tumor from the head of Mary Vincent, of Jackson, Ga.

4th, Jan. 8th, 1845, in the amputation of a finger of a negro boy belonging to Ralph Bailey, of Jackson, Ga.

Copies of the letters and depositions proving these operations with ether were all shown me by Dr. Long. * * *

I then called on Profs. Joseph and John Le Conte, then of the University of Georgia, at Athens, and inquired if they knew Dr. Long, and what his character was for truth and veracity. They both assured me that they knew him well, and that no one who knew him in that town would doubt his word, and that he was an honorable man in all respects.

Subsequently, on revisiting Athens, Dr. Long showed me his folio journal, or account book, in which stand the following entries:

'James Venable
March 30th, 1842, Ether and excising tumor, $2.00
May 13th, Sul. Ether, .25
June 6th, Excising tumor, 2.00'

On the upper half of the same page, several charges for ether sold to the teacher of the Jefferson Academy are recorded, which ether Dr. Long told me was used by the teacher in exhibiting its exhilarating effects, and he said the boys used it for the same purpose in the academy. I observed that all these records bore the appearance of old and original entries in the book.

On asking Dr. Long why he did not write to me or make known what he had done, he said, when he saw my dates, he perceived that I made the *discovery* before him, and he did not suppose that anything done after that would be considered of much importance, and that he was awakened to the idea of asserting his claims to the first practical *use* of ether in operations, by learning that such claims were set up by others for this merit, and consequently he wrote to the Georgia delegation at Washington, stating the facts which Senator Dawson had requested me to inquire into.

I have waited expecting Dr. Long to publish his statements and evidence in full, and therefore have not before published what I learned from him. He is a very modest, retiring man and not disposed to bring his claims before any but a medical or scientific tribunal. * * *

Had he written to me in season I would have presented his claims to the Academy of Sciences of France, but he allowed his case to go by default, and the academy knew no more of his claims to the practical use of ether in surgical operations than I did.

Boston, *April* 3, 1861. Charles T. Jackson, M. D."

The list of operations as given by Dr. Jackson is not complete, as he has omitted the second operation on Venable, and a number of the later operations. In a letter to Dr. Sims, which I have, Dr. Long denies absolutely that he ever acknowledged that Dr. Jackson was the prior discoverer. He had been led to infer that ether had anæsthetic powers several months before he got a chance to verify it, and before Jackson claims to have made similar inferences, but he dated his claims of discovery from the time of his first practical demonstration. Before that it was a mere supposition, as was Jackson's also.

But barring these inaccuracies, Dr. Jackson's paper, coming as it does from one who so zealously coveted the title of discoverer, is a remarkable admission.

The interview between Long and Jackson must have been most amicable, and Long evidently felt the greatest respect for Jackson, as shown in the following letter:

"Athens, Ga., Nov. 15th, 1854.

Dr. C. T. Jackson.

Dear Sir:—I design to prepare an article with the proofs of the priority of my claims of the discovery of the anæsthetic powers of ether and of its applicability to surgical operations. I design having this published in pamphlet form for distribution among the members of the medical profession, and I expect to present such proof with the article as will satisfy all that I am entitled to all I claim.

Ours are rival claims, and permit me, sir, to say that although our claims are conflicting, I would not knowingly say anything in the article which would be displeasing to you. I entertain high respect for you as a gentleman and man of science and feel honored by your acquaintance.

Still it becomes each one of us to use all honorable means to advance his own claims, and I know you will not blame me for attending to this matter, which so much concerns my reputation.

Shall it meet with your approbation, I may refer to your admissions to Hon. W. C. Dawson and myself, of the belief of the correctness of my claims. I will, however, make no allusion to your letter to Mr. Dawson or to the conversation held with myself unless it meets with your sanction. . . . .

Your obedient servant, C. W. Long."

Morton's Patent.

It has often been stated by the friends of Morton that he never attempted to enforce his patent. This statement has lately been reiterated by his wife in McClure's Magazine. On this point the following letter from a prominent army surgeon to Long may be of interest:

"U. S. Marine Hospital,
Chelsea, Mass., April, 1859.

Dr. Crawford W. Long, Athens, Ga.

Sir:—Hon. Judge Hyllier, Solicitor of Treasury Department, informed me about a year since, and recently repeated the same, that some years since you used sulph. ether as an anæsthetic and had a record of the same. If it is not asking too much of you, I would be greatly obliged if at your earliest convenience you would forward me a statement of the facts.

I take the liberty to ask this of you because Mr. W. T. G. Morton, to whom in conjunction with Dr. C. T. Jackson a patent was granted in Nov., 1846, for using ether, has brought a suit against me as a government officer for an infringement of his patent.

Judge Hyllier was confident that you could furnish me with proof sufficient to satisfy a jury that you used it way before he or Jackson claimed to have made the discovery. I should have asked for these proofs through my attorney and had them properly witnessed, etc., but the Secretary of the Treasury having decided that I used the article on my own responsibility and therefore the Govt. were not bound to defend me, I wish to save as much expense as possible. Very respectfully,

[Signed] Charles A. Davis, M. D.,
Physician and Superintendent."

In reply Long gives a detailed account of his work, and then adds: "I presume Dr. Jackson is not party to the prosecution, as I know he entertains no good feelings towards W. T. Morton. If you think proper you can see him and ascertain the character of proof I can make. From the little acquaintance formed with him I entertain a high opinion of him as a gentleman and think he will do me justice notwithstanding he himself claims to have made the discovery and has received several awards."

Dr. Davis's letter was written twelve years after Morton's "Letheon" was patented, and many years after ether was the common property of the world. It was probably Morton's last attempt to get money from the U. S. Government.

The ether controversy was never reopened and Long's work was unknown to the world until 1877, when J. Marion Sims learning of him through accident, investigated his claims, was fully convinced of their merit, and vigorously demanded their recognition by the medical profession. His paper appeared in the Virginia Medical Monthly, May, 1877.

The Wilhite Claims.

This article, which obtained for Long the first recognition of any consequence, was the outcome of a conversation which Sims had with a Dr. P. A. Wilhite, of Anderson, S. C. Wilhite told Sims that he had witnessed the first surgical operation ever done under ether, and recounted Dr. Long's

first case, saying that he was one of four students who were then in Dr. Long's office.

He also said that "he presumed that he was the first person who ever profoundly etherized any one," and told how he was at a quilting party in which the boys and girls had concluded the evening by inhaling ether for sport; how they had caught a negro boy, and as he refused to inhale the ether, had firmly held a handkerchief soaked with ether over his face, when to their horror his breathing became stertorous and he could not be aroused. He then vividly pictured their alarm when they realized that they had murdered a fellow-being, and their determination to leave the country, which was prevented by the timely arrival of a physician who restored the boy to consciousness.

Wilhite then related how when the idea of using ether to prevent pain in surgical operations had occurred to Long, *he had encouraged him by relating the story of the negro boy.*

Dr. Sims at once communicated with Dr. Long and soon convinced himself of the truth of his claims, but unfortunately he failed to investigate Wilhite's statements, but embodied them in full in his article, giving to Wilhite the credit of first intentionally producing profound anæsthesia with ether.

Dr. Sims' paper was given great publicity and the Wilhite story has been accepted as true, and copied by many writers on the subject, most notably by Sir Jas. Paget.*

The negro boy story lacks probability, as Wilhite did not enter Long's office until 1844, two years after the first operation, as the following letter from Long to Wilhite shows:

"ATHENS, GA., May 20th, 1877.

DR. P. A. WILHITE.

Dear Sir:—I received Dr. Sims' article on anæsthesia yesterday and find several mistakes. Dr. Sims states that yourself, Dr. Groves, and Drs. J. D. and H. R. J. Long were students of mine and witnessed the operation performed on Venable in 1842. Your recollection failed you at the time. As it was several years, at least two, before either entered my office, you will see that you were mistaken in giving Dr. Sims this information. You also make a mistake in saying that the first inhalation in Jefferson of ether for its exhilarating effects was before the same persons. * * * I wrote to Dr. Sims informing him of the errors and asking him if he considered the mistakes of sufficient importance to be noticed, etc. (Signed) C. W. LONG."

Dr. Wilhite replied as follows:

"ANDERSON, S. C., June 27, '77.

DR. C. W. LONG.

Dear Doctor:—Yours of the 22nd inst. is at hand, and I have also just received a letter from Dr. J. M. Sims, which I will answer to-day. * * *

In my statement I did make a mistake in regard to my being present at the first or second operation, which mistake I will correct. But if you still prefer I will send a certificate. * * *

Let me know and I will give you any information or assistance in this great matter. Yours truly, etc.,

(Signed) P. A. WILHITE."

In the letter to Wilhite, Long makes no comment on the negro-boy incident, but his daughter informs me that he repeatedly told her that he had never heard of it before it appeared in Sims' article.

It is to be regretted that the justification of Long's claims should have been linked so closely with such misstatements.

Sims sailed for Europe soon after the publication of his article, and Long died in a few months, and Wilhite's statements went unchallenged for many years.*

Among Dr. Long's papers are many more documents—affidavits of persons operated on and witnesses to them, and letters to different prominent men—but their recital would take up too much time, so I will close with a few words regarding his after life.

In 1842 Dr. Long was married to Miss Caroline Swain, a niece of Governor Swain of North Carolina, a very handsome and attractive woman, who proved a devoted wife. She survived her husband many years.

Long remained in Jefferson for ten years, when he removed to Athens and there spent the rest of his days. By inheritance and professional labors Dr. Long had amassed a fortune, which was largely swept away by the war of secession, and at its close he found himself reduced to poverty, with a large family to support.

Although he soon regained an extensive practice, the desolation of the country and the general poverty of the people made the remainder of his life a continual struggle against poverty. His life, which, up to the time of Dr. Sims' article, had been one of disappointments, after that suddenly became brighter, for from all parts of the world men prominent in the medical profession hastened to give him the credit which had so long been withheld.

His claims were never investigated by the American Medical Association, as he often desired, but many minor societies and the "Eclectic" Medical Association passed decrees in his favor, and a statue was erected in his honor in Paris, France.

But he was not long to enjoy the praise and long-delayed honors which were now heaped upon him. Within a few months, while laboring at the bedside of a delicate patient, he was stricken with apoplexy and died the next day, June 16th, 1878, at the age of sixty-two, poor in worldly goods but rich in the gratitude of his people. His oft-repeated wish to die in harness had been granted.

A strange fatality seemed to hang over the lives of all connected with the discovery of anæsthesia!

Wells, disappointed and disheartened by the rejection of his claims by the French Academy, became insane and committed suicide in 1848.

Morton gave up a very lucrative practice and vainly spent his life in trying to enforce his patent and get a reward from Congress. He died in 1868 from congestion of the brain, brought on by excitement occasioned by an article attempting to deprive him of the honor he so jealously coveted.

Long died in poverty, from apoplexy brought on by overwork in 1878.

Jackson, like Wells, became insane from the bitter contentions of his life and died in an asylum in 1880.

*The Nineteenth Century, 1880.

*In an article in the Virginia Medical Monthly, 1893, Dr. L. B. Grandy, of Atlanta, showed the error of Wilhite's statements.

Henri L. Stuart, founder of the Woman's Hospital, and a great New York philanthropist, became interested in Long's claims and presented a portrait of him to the University of Georgia. After seeing it unveiled with great ceremony in the capitol in Atlanta, one year after Long's death, he went to Athens as a guest of the Longs, to visit the grave of the discoverer of anæsthesia. Arriving at night, he waited till morning to fulfil his heart's desire. But this was never gratified. During the night he had a paralytic stroke and died at the home of the Longs after lingering several weeks. At his own request his remains were interred next to those of Crawford Long—two great benefactors side by side.

Appendix I.

Dr. Long's Original Paper.*

In the month of Dec., 1841, or Jan., 1842, the subject of the inhalation of nitrous oxide gas was introduced in a company of young men assembled at night in the village of Jefferson, Ga., and the party requested me to prepare them some. I informed them I had not the requisite apparatus for preparing or preserving the gas, but that I had an article (sul. ether) which would produce equally exhilarating effects and was as safe. The company were anxious to witness its effects, the ether was introduced and all present in turn inhaled. They were so much pleased with its effects that they afterwards frequently used it and induced others to do the same, and the practice soon became quite fashionable in the county and some of the contiguous counties.

On numerous occasions I inhaled ether for its exhilarating properties, and would frequently, at some short time subsequent to its inhalation, discover bruised or painful spots on my person which I had no recollection of causing and which I felt satisfied were received while under the influence of ether. I noticed my friends while etherized received falls and blows which I believed were sufficient to produce pain on a person not in a state of anæsthesia, and on questioning them they uniformly assured me that they did not feel the least pain from these accidents. Observing these facts I was led to believe that anæsthesia was produced by the inhalation of ether, and that its use would be applicable in surgical operations.

The first patient to whom I administered ether in a surgical operation was Mr. James M. Venable, who then resided within two miles of Jefferson, and at present lives in Cobb Co., Ga. Mr. Venable consulted me on several occasions in regard to the propriety of removing two small tumors situated on the back part of his neck, but would postpone from time to time having the operation performed, from dread of pain. At length I mentioned to him the fact of my receiving bruises while under the influence of the vapor of ether without suffering, and as I knew him to be fond of and accustomed to inhale ether, I suggested to him the probability that the operations might be performed without pain, and proposed operating on him while under its influence. He consented to have one tumor removed, and the operation was performed the same evening. The ether was given to Mr. Venable on a towel, and when fully under its influence I extirpated the tumor.

It was encysted and about half an inch in diameter. The patient continued to inhale ether during the time of the operation, and when informed it was over, seemed incredulous until the tumor was shown him.

He gave no evidence of suffering during the operation, and assured me, after it was over, that he did not experience the least degree of pain from its performance. This operation was performed on the 30th March, 1842.

The second I performed on a patient etherized was on the 6th June, 1842, and was on the same person, for the removal of the other small tumor. This operation required more time than the first, from the cyst of the tumor having formed adhesions to the adjoining parts.

The patient was insensible to pain during the operation until the last attachment of the cyst was separated, when he exhibited signs of slight suffering, but asserted after the operation was over that the sensation of pain was so slight as scarcely to be perceived. In this operation the inhalation of ether ceased before the first incision was made. Since that time I have invariably desired patients, when practicable, to continue the inhalation during the time of the operation.

Having permitted such a length of time to elapse without making public my experiments in etherization, in order to show the correctness of my statements I procured the certificate of the patient on whom the first operation was performed, the certificate of two who were present at the time of the operation, and also those of his mother, brothers and sisters and a number of his immediate friends who heard him speak of the operations soon after they were performed. The Southern Med. and Surg. Journal* contained but two of the certificates. I have a number of others which can be seen or read if desired by the Society. My third case was a negro boy who had a disease of a toe which rendered amputation necessary, and the operation was performed July 3rd, 1842, without the boy evincing the slightest sign of pain.

These were all the surgical operations performed by me during the year 1842 upon patients etherized, no other case occurring in which I believed the inhalation of ether applicable. Since '42 I have performed one or more surgical operations annually, on patients in a state of etherization.

I procured some certificates in regard to these operations, but not with the same particularity as in regard to the first operations, from the fact of my sole object in the publication being to establish my claim to priority of discovery of power of ether to produce anæsthesia. However, these certificates can be examined.

The reasons which influenced me in not publishing earlier are as follows:

I was anxious, before making my publication, to try etherization in a sufficient number of cases to fully satisfy my mind that anæsthesia was produced by the ether, and was not the effect of the imagination or owing to any peculiar insusceptibility to pain in the persons experimented on.

At the time I was experimenting with ether there were physicians high in authority and of justly distinguished character who were the advocates of mesmerism, and recommended the induction of the *mesmeric state* as adequate to prevent pain in surgical operations. Notwithstanding thus sanctioned I was an unbeliever in the science, and of the opinion that if the mesmeric state could be produced at all it was only on those of strong imaginations and weak minds, and was to be ascribed solely to the workings of the patient's imagination. Entertaining this opinion, I was the more particular in my experiments in etherization.

Surgical operations are not of frequent occurrence in a country practice, and especially in the practice of a young physician, yet I was fortunate enough to meet with two cases in which I could satisfactorily test the anæsthetic power of ether. From one of these patients I removed three tumors the same day; the inhalation of ether was used only in the second operation, and was effectual in preventing pain, while the patient suffered severely from the extirpation of the other tumors. In the other case I amputated two fingers of a negro boy; the boy was etherized during one amputation and not during the other; he suffered from one operation and was insensible during the other.

After fully satisfying myself of the power of ether to produce anæsthesia, I was desirous of administering it in a severer surgical

* Read before Georgia State Medical Society in 1852.

* Dec. 1849.

The first patient to whom I administered ether in a surgical operation was Mr James M. Venable who then resided within two miles of Jefferson and at present lives in Cobb Co Ga — Mr Venable consulted me on several occasions in regard to the propriety of removing two small tumors situated on the back part of his neck, but would postpone from time to time having the operation performed from dread of pain — At length I mentioned to him the fact of my having inhaled ether while under the influence of it, without suffering and as I knew him to be fond of and accustomed to inhale ether, I suggested to him the probability that this operation might be performed without pain and proposed operating on him while under its influence — He consented to have the tumors removed and the operation was performed the same evening — The ether was given to Mr Venable on a towel and when fully under its influence I excised the tumors — It was encysted and about half an inch in diameter. The patient continued to inhale ether during the time of the operation and, when informed it was over, seemed incredulous, until the tumors were shown him — He gave no evidence of suffering during the operation, and assured me, after it was over, that he did not experience the least degree of pain from its performance — Thus operation was performed on the 30th March 1842.

operation than any I had performed. In my practice, prior to the published account of the use of ether as an anæsthetic, I had no opportunity of experimenting with it in a capital operation, my cases being confined, with one exception, to the extirpation of small tumors and the amputation of fingers and toes.

While cautiously experimenting with ether, as cases occurred, with the view of fully testing its anæsthetic powers and its applicability to severe as well as minor surgical operations, others more favorably situated engaged in similar experiments, and consequently the publication of etherization did not "bide my time."

I know that I deferred the publication too long to receive any honor from the priority of discovery, but having by the persuasion of my friends presented my claim before the profession, I prefer that its correctness be fully investigated before the Med. Society. Should the society say that the claim, though well founded, is forfeited by not being presented earlier, I will cheerfully respond, so mote it be.

Not wishing to intrude upon the time of the Society, I have made this short compendium of all the material points stated in my article in the Journal, and if the Society wishes any fuller information on the subject I will cheerfully comply with their wishes.

Appendix II.

"Georgia, Jackson County. } I, Ange De Laperriere, M. D., do certify that I resided in Jefferson, Jackson County, Georgia, in the year 1842, and that some time in that year I heard James M. Venable, then of said county, speak of Dr. C. W. Long's cutting out two tumours from his neck while under the influence of the inhalation of sulphuric ether, without pain or being conscious of the performance of the operation.

I do further certify that the fact of Dr. C. W. Long using sulphuric ether by inhalation to prevent pain in surgical operations was frequently spoken of and notorious in the county of Jackson, Georgia, in the year of 1842. A. De Laperriere, M. D.

Sworn to and subscribed before me this 30th of March, 1854.
N. H. Pendergrass, J. P."

"Athens, Clarke Co., Georgia.

I, the undersigned, do certify that in May, 1843, I assisted Dr. R. D. Moore in amputating the leg of a colored boy Augustus, then the property of Mr. Wm. Stroud, who resided in this county; and that I distinctly recollect hearing Dr. R. D. Moore say, If I had thought of it before leaving home I would have tried Dr. C. W. Long's great discovery, namely, the administration of sulphuric ether as an anæsthetic in performing the operation. Having neglected to bring the ether, Dr. Moore finally concluded to influence the patient with morphia; under which influence the operation was performed. Jos. B. Carlton, M. D."

Appendix III.

From Dr. Long's First Student.

"Cohutta, Ga., Dec. 13th, 1894.

Mrs. Frances Long Taylor,

Dear Madam:—* * * In 1844, soon after I attained my majority, I decided to adopt medicine as my profession, and began to think where and under whom I should begin the preparatory study. My father asked me to choose from among the number of physicians whom I knew the one I preferred to act as preceptor to me.

Knowing Dr. Long so well and believing him to be a man of no ordinary ability, I at once fixed upon him as my choice.

I entered Dr. Long's office in May, 1844, as the first student ever under his care. As I progressed with my studies he saw fit to make known to me his discovery, by the use of which he could perform surgical operations without giving any pain to his patient. [Here follows a description of the first cases.]

Not satisfied, however, that there was not more to learn about this great discovery, he proposed that we test it further personally, which we did in his office, where with closed doors we administered it to each other to prove its perfect anæsthetic effect and also to discover any bad effect to the subject etherized. Owing to the prejudice and ignorance of the populace Dr. Long was prevented from using ether in as many cases as he might have.

Thus in the two years preceding my entering Dr. Long's office he had had only about six cases in which to try the anæsthetic effects of ether.

The first case that came under his care where its use was applicable after my going into his office was not till January 8th, 1845, which was the case of a negro boy having two fingers to amputate, caused by neglected burn. I, as the only student still with the doctor, he had me to accompany him to see the operation and assist *in the administration of the ether*. The first finger was removed while under the influence of ether, the little fellow evincing no pain; the second without ether, the child suffered extremely. This was done to prove that insensibility to pain was due to the agent used.

Soon after this, in January, Mr. J. D. Long came into the office as a fellow-student; *later, toward spring, came P. A. Wilhite*, and in August came Dr. Long's brother, H. R. J. Long. We four remained there at Dr. Long's office as students until the opening of the fall term of the medical colleges. * * *

[Signed] J. F. Groves, M. D.

Sworn and subscribed to before me, Dec. 15th, 1894.
Wm. H. Wilson, N. P."

THE EARLY HISTORY OF OPHTHALMOLOGY AND OTOLOGY IN BALTIMORE (1800–1850).*

By Harry Friedenwald, A. B., M. D., *Associate Professor of Ophthalmology and Otology, College of Physicians and Surgeons, Baltimore, Md.*

An old book tells us that each generation may be looked upon as standing on the shoulders of its fathers. If its vision is clearer, its intellectual view less obstructed, its horizon broader, it is in great part due to the height to which others have raised it, to the support others have given. Unmindful of this, it is apt to exaggerate its greatness and the importance of its own work.

In the following narrative I have brought together all that I could find relating to the lives and labors of those who, in the earlier years of this century and in our own city, tilled the soil of ophthalmology and otology. Some have been forgotten, few have been accorded deserved recognition.

Are not many of us as ignorant of their names and works as an old physician from whose memories I had hoped to obtain information, but whose response was, "No work was done in Baltimore in those departments of medicine before 1850"? I must confess that when my attention was first drawn to this

*Read before the Johns Hopkins Hospital Historical Club, April, 1897.

subject I knew of but one work of importance which a Baltimore physician had rendered to ophthalmology.

There is no reference in literature to anything done in Baltimore in the two branches we are considering before the beginning of this century. We must remember that in 1800 the population of Baltimore City was 26,614 and that in 1802 there were but 44 physicians.

It is probable that there were here as elsewhere those who confined themselves to diseases of the eye. This is indicated by a resolution adopted at the convention of the Medical and Chirurgical Faculty of Maryland in 1805.* It was resolved that "the Board of Examiners be authorised to grant special licenses to dentists and occulists to practice in their respective branches, subjecting them to an examination only on the branches they possess; and that such licentiates shall pay ten dollars for each license so obtained; provided it shall be the opinion of the attorney-general that the law authorizes the examiners to grant such licenses. The secretary having submitted the preceding resolution to the then attorney-general, he gave it as his opinion that the law authorized the board to act according to the spirit of the resolution." These "occulists" did not, it appears, stand in very good repute.

In the review in 1825 of an American treatise on diseases of the eye, the writer, who was probably Dr. Isaac Hays, the distinguished editor of the American Journal of Medical Sciences and an ophthalmologist of note, describes the condition of ophthalmology in the early years of this century. He tells us that "the neglect to which disorders of the eye were too long consigned is truly astonishing. Prepossessed with an idea that there was something peculiar in the diseases attacking different parts, physicians entirely abandoned these affections to persons who were exclusively devoted to them and were totally ignorant of the laws which influence diseases of other organs. To this cause is to be attributed the slow progress which the science of ophthalmology made during many centuries. The history of this science shows that while it made most rapid advances in improvement by the investigations of medical men, it invariably, when abandoned to professed oculists, not only ceased to advance but actually retrograded . . . In this country much apathy has existed, and we fear still exists, with regard to these affections. Some of our distinguished surgeons have not, it is true, entirely neglected them; yet the mass of medical practitioners have paid little attention to them, and the science has advanced so rapidly during the few years that we believe few have kept pace with the improvements that have been made. The length of time that generally elapses before opinions of European writers are diffused in this country—the want of regular lectures—and above all, clinical instruction and the opportunities furnished of observing these diseases in institutions established for the reception of these cases, are the cause, no doubt, of the present state of the science among us."

He mentions that the New York Eye Infirmary was established in 1820, the Pennsylvania Infirmary for Diseases of the Eye and Ear in 1822, and "with respect to the institution at Baltimore he has but little information to communicate. It is attached to the Baltimore Dispensary and is committed to the care of the author of this work." [Dr. Geo. Frick.]

*Summary of Proceedings, &c., published in 1817.

PIERRE CHATARD.

The earliest Baltimore publication having reference to diseases of the eye is found in a paper written by Dr. Pierre Chatard in the Medical Repository, vol. VII, p. 28. Dr. Chatard was born and educated in France, and had settled in Baltimore in 1797. He was a prolific writer, the paper referred to being one of the earliest. It was entitled "An account of a case of Fistula Lachrymalis, with reflections on the different modes of operating in that disease." The paper describes a case of lachrymal fistula relieved by introducing threads of silk after the manner of a seton through the lachrymal duct and gradually increasing their number. He discusses at length the various methods in vogue at that time of treating the disease. It is written in an interesting and elegant manner. None of the other writings of Chatard relate to diseases of the eye.

WILLIAM GIBSON.

During the second decade of this century the celebrated surgeon, Wm. Gibson, practiced in Baltimore. Wm. Gibson was born in 1784 in Baltimore, and was graduated in medicine in Edinburgh in 1809. He was a physician to the Baltimore General Dispensary in 1818–19, and professor of surgery at the University of Maryland from 1812–19, resigning to occupy the same chair at the University of Pennsylvania from 1819–54. He died about 1858 (?).

His numerous publications date mainly from the latter period of his activity, but there are two notable and very interesting references to ophthalmic surgery dating from the Baltimore period.

In Hirsch's History of Ophthalmology* we find the statement that Prof. Gibson of Baltimore had made the experiment of introducing a seton through the cataractous lens, with the view of producing its absorption, so original an experiment that Hirsch cites the description given by Dr. John Revere untranslated. My friend, Dr. O'Connor of Boston, had the kindness of looking up the reference in the New England Journal of Medicine, vol. VIII, p. 119, of 1819, and I will give this account in full because of its intense interest.

Extract of a letter to one of the editors:

"I am desirous of communicating through the medium of the New England Journal a new mode of operating for cataract, which has been projected and practiced recently, *in two cases*, with the most satisfactory success, by my friend, Dr. Gibson, professor of surgery at the University of Maryland.

The operation was performed in the following manner: The iris was in the first place dilated by the application of atropa belladonna. A common sewing needle, slightly curved and armed with a single thread of silk, was then passed through the tunica sclerotica about two lines from the cornea, where the couching needle is usually introduced, through the opaque lens and out of the opposite side of the cornea, at a point corresponding to the one at which it was introduced. The silk being drawn through, and the ends cut off, a single thread was thus left passing through the ball of the eye, and acting on the diseased lens in the manner of a seton. It

*Graefe and Saemisch's Handbuch, Vol. VII, p. 517.

was feared that serious inconvenience might arise from the irritation produced upon the tunica conjunctiva, from the excessive sensibility of this membrane. Fortunately, however, neither this nor any other accident intervened, and at the end of ten days, in both cases the diseased lens had disappeared, and, in its place, the silk was distinctly seen passing like a bar across the pupil of the eye. The silk was withdrawn, and in a few days the vision was restored. In the third and last case in which this operation was performed it failed in consequence of the iris being wounded. This caused such an inflammation of the organ that it was deemed proper to withdraw the seton at a very early period. This accident was attributed to not using the belladonna. One would think that a common sewing needle is not the most convenient instrument that could be devised for this purpose, on account of the dense structure of the part through which it is to pass, and the difficulty of having the perfect command of any instrument without a handle. From the nature of the disease, from the known effects of this remedy when applied to other parts of the body, and from the success which has already been experienced, there seems to be good reason to hope that this will be found an important improvement on the established practice in many forms, if not in every variety of the disease. But the practical benefit to be derived from this operation can only be tested by a more enlarged observation than in this country ever falls to the lot of any individual.

I remain yours, J. Revere.

Balto., Mch. 2nd, 1819."

We learn from Hirsch that Loewenhardt in 1828 operated in a similar manner for secondary cataract with anterior and posterior synechiæ, likewise with good result; it appears that others have not attempted the operation.

In the American Appendix to the second edition of Cooper's Dictionary of Practical Surgery, edited in America in 1844 by David Meredith Reese, who in 1842 and 1843 was professor in Washington University of Baltimore, we find a very curious reference to Professor Gibson under the head of strabismus: "It appears from the Institutes of Surgery that Professor Gibson attempted the cure of strabismus by dividing the recti muscles of the eye, precisely as now practiced, some twenty years since, in Baltimore. Soon after he repeated it unsuccessfully in Philadelphia in several cases, and was induced to abandon it by unfavorable opinions expressed on the operation by Dr. Physick. He, however, inculcated the propriety of the operation upon his class many years since, and Dr. A. E. Hosack, of New York, then one of his pupils, distinctly recollects Dr. Gibson's expressions of confidence that the operation would ultimately succeed."

I had no little difficulty in finding the reference to which Dr. Reese refers. The first five editions of Gibson's Institutes of Surgery (published between 1824 and 1838) contain no chapter devoted to strabismus, and in the seventh edition the long account of strabismus and its treatment consists almost entirely in a paper written by Charles Bell and sent by him to Gibson. In this chapter the above reference is likewise not to be found. It is only in the sixth edition, published in 1841, that we find the reference. It is of such importance that I shall give it in detail. On page 375 Gibson states that:

"In the year 1818, while practising my profession extensively in Baltimore, the late Mr. B. J. consulted me about his daughter, a child of eleven or twelve years of age, both of whose eyes were directed very much inwards, and were thereby greatly deformed by a squint. I advised a pair of goggles, so contrived, by having a small opening in the centre of each, as to oblige the child to direct the cornea to these openings, and by perseverance for several weeks, succeeded in diminishing the deformity but not effecting a cure. In the course of my visits the child remarked at different times that her eyes felt as if tied by a string. Struck with this observation, and conceiving the disease might depend upon shortening of the internal rectus muscle, I determined, the first opportunity, to try the result of divison of that muscle; and as the friends of my young patient were unwilling the experiment should be first tried upon her, I selected a hospital patient, and after some difficulty in fixing the eyeball and in cutting the muscle across, succeeded in restoring the eye partially to its natural situation. Upon two other patients I repeated the experiment, without much better success, but on dividing a muscle in a fourth patient, after my removal to Philadelphia, the eye was so completely turned to the opposite direction as to bury the cornea beneath the lids and create a much greater deformity than had previously existed. Upon showing the patient to Dr. Physick, he advised the experiments to be abandoned, as likely to be followed by very unfavorable results. I mention these circumstances, not from a desire to receive credit as an inventor or to detract from the claims of the distinguished surgeon with whom the modern operation of strabismus originated, but merely as a curious fact, calculating to show the importance of not laying aside processes apparently founded upon correct principles, simply because we are at first foiled in our attempts to execute them. How much benefit would have resulted to the community if I had followed up my operations until I ascertained the proper mode of correcting them, or how much injury I might have inflicted upon individuals by perseverance in the attempt, I shall not stop to inquire. It is sufficient for me to announce the fact—which I have no doubt could be easily substantiated by many pupils who attended my early lectures, some of whom have indeed already proffered their testimony—without being over-solicitous, in setting up a claim as an inventor, of exposing my awkwardness and perhaps want of knowledge of the principles that should have guided me in following out the practice I had attempted to institute."

It may not be out of place to mention that Stromeyer's important monograph, in which he recommended division of the muscle for strabismus appeared in 1838!

The chapters devoted to diseases of the eye in Gibson's Institutes are important, but as the book appeared after the author had left Baltimore, it is not proper to discuss them here.

Another reference which shows Gibson's interest in the eye occurs in the American Medical Recorder (vol. II, p. 283). It is a "Letter of Charles Bell to Professor Gibson, of Baltimore," and its subject is, "The New Coat of the Eye discovered by McCarthy's demonstrator, Jacobs."

George Frick.*

We may next take up the most important name of this narrative, that of Dr. George Frick, the author of a valuable treatise on diseases of the eye, the first work of the kind that appeared in America.

George Frick was born in Baltimore in 1793. After obtaining a broad classical education he entered the University of Pennsylvania, where he obtained the degree of doctor of medi-

*The accompanying portrait of Dr. Frick is copied from one recently presented to the Medical and Chirurgical Faculty, together with a case of instruments which belonged to the doctor, by his niece, Mrs. White and her daughter, Miss Mary White.

cine in 1815, and in 1817 he was admitted as licentiate of medicine into the Medical and Chirurgical Faculty of Maryland. He then spent several years abroad, returning to Baltimore about 1819 to engage in the practice of ophthalmology. He was appointed surgeon to the Baltimore General Dispensary, where he established the first Eye Dispensary in Baltimore, in 1824. In 1822 he delivered clinical lectures at the Maryland Hospital. His name is found on the list of vaccine physicians for 1821.

He was a member of various medical societies; was secretary of the Faculty in 1823, and joined the Maryland Medical Society in 1822. He was much interested in general science, and was one of four physicians to organize a society for promoting science, in 1819. He was likewise a member of the Maryland Academy of Sciences, its librarian in 1824 and curator in 1836.

He devoted himself to the practice of ophthalmology and to the cultivation of general scientific studies, as well as to music for a number of years. He was unfortunate in growing very deaf before middle life, and it is probable that this interfered greatly with his practice of medicine; for somewhere about 1840 he entirely relinquished it and left Baltimore to spend most of his time in Europe, paying occasional visits to this country. He died in Dresden, March 26th, 1870, aged 77 years. Dr. Frick had never married. He was a man of very retiring and modest character and of kind disposition. He was a careful scientific student and his work and writings deserve high praise.

DR. GEORGE FRICK.

His first writing was his thesis for the degree in medicine; its subject was "On the Meloe Vesicatorium" (1815). In 1820–21 his article on "Observations on Cataract and the various modes of operating for its cure" appeared in the American Medical Recorder of Philadelphia. These articles cover over 40 pages. In 1821 an article on "Observation of the various forms of Conjunctivitis" appeared in the same journal, and in 1823 his paper on "Observation on Artificial Pupil and the modes of operating for its cure."* His most important work, however, was "A Treatise on the Diseases of the Eye; including the doctrines and practice of the most eminent modern surgeons and particularly those of Professor Beer," which was published in Baltimore by Fielding Lucas, Jr., in 1823. It was inscribed to his teacher, Dr. Physick of Philadelphia. The articles above referred to, though somewhat more elaborate, were in the main identical with the corresponding chapters of the treatise and do not therefore require special consideration. The treatise is of considerable value.* It is well and clearly written, the system upon which it is classified is excellent, and no greater praise could be given it than stating the fact that it was republished three years later in London by an English surgeon, Richard Welbank, a member of the Royal College of Surgeons and of the Medical and Chirurgical Society of London, and dedicated to the ophthalmologist William Lawrence. Numerous foot-notes were added, but the text suffered no change.

The reviews which the book received were very complimentary. The Philadelphia Journal of Medical and Physical Sciences (probably Dr. Isaac Hays) contains a review covering 18 pages: "The author evidently possesses a cultivated and well disciplined mind; he appears to be intimately familiar with German writers, and we feel much indebted to him for making us acquainted with their writings."

In the American Medical Recorder of 1824 a still longer review is to be found, covering 32 pages. The writer describes the book in terms of high praise. He "offers the humble tribute of (his) thanks to the author for the benefits which he has conferred on the profession generally, by presenting them with a volume of great value and utility, and one which was much

* This curious error is cited as it is found.

* It is interesting to find numerous pencil notes in the articles in the copies of the American Medical Recorder at the library of the Medical and Chirurgical Faculty of Maryland, notes suggesting slight changes in the phraseology, paragraphing, etc., every one of which has been adopted in the treatise. I have been able to trace this copy, which was bought with other books by Dr. John Morris at a public sale of the library of Dr. John Buckler, who was related by marriage to Dr. Frick, and whose library thus passed into Dr. Buckler's. Numerous books at our library contain Dr. Frick's autograph, and one, a copy of Gibson's Institutes of Surgery, has an inscription of the author to his friend Dr. Frick. In Beer's work on ophthalmology there are interesting pencil notes and several pages of written matter which correspond so thoroughly with Dr. Frick's writing that I have no hesitation in stating that they are his.

wanted. As a manual of the diseases of the eye, we believe it to be the best which has been published. It contains all the improvements which have enriched ophthalmic surgery, in such a surprising degree within a few years past," etc.

In 1825 this work was placed on the list of those which the student was required to have read before applying to the Medical and Chirurgical Faculty of Maryland for the examination for licentiate in medicine.

Hirsch in his history of ophthalmology says that "George Frick was the apostle of the ophthalmological school of Vienna in North America; his treatise was next to Saunders's, the first large treatise on ophthalmology in America, and was received by physicians with great praise." Saunders's book was an English work and was republished in Philadelphia in 1821, two years before Frick's. The only other book in the English language of a similar kind was that of Travers, which appeared in London in 1820-21-24.

We thus see that Frick's book was the first American treatise (and for a number of years it remained alone). The work is much quoted in Cooper's Dictionary of Surgery.

In Quinan's Medical Annals of Baltimore I find a reference to a paper of Dr. Frick's "On the Senses," which he tells us was read before the Medical Society of Maryland in 1821. I have been unable to find this paper.

In Cordell's History of the University of Maryland we learn that the foundation of the Infirmary was laid in 1823 and that patients were received in the same year. Of the four wards, "one was reserved for eye cases, instruction in ophthalmic surgery forming a prominent feature in the course. This was during the time of Frick's greatest activity, and it is possible that the prominence given to ophthalmology was through him, and that he delivered the clinical lectures in this branch. I am unable to verify this.

In conclusion it is interesting to call attention to the fact that Dr. George Frick was the uncle of the distinguished clinician, Professor Charles Frick.

Horatio G. Jameson.

Horatio G. Jameson, born in Pennsylvania about 1792, graduated in medicine at the University of Maryland in 1813. He held a number of important public positions, having been consulting surgeon of the Baltimore City Hospital from 1819 to 1835, consulting physician of the board of health of Baltimore City in 1827-35; he was incorporator of the Washington Medical University in 1827, and professor of surgery and surgical anatomy in the same from 1827-35, when he became professor of surgery in the Cincinnati Medical College. He was one of the most prominent surgeons of Baltimore for a number of years, and a very active contributor to medical journals, writing important papers in medicine and in surgery. He was the editor of the Maryland Medical Recorder during its existence of several years (Sept. 1829-Nov. 1832), and a large number of its articles are from his pen.

Jameson appears to have been much interested in diseases of the eye. We find frequent references to publications on these subjects throughout his journal, several of which have remarks added by the editor. In vol. 2 there is an article on the "Pathological Sympathy between Eye and Larynx" (p. 117). This article is without any value, indeed it is difficult to understand the real significance of his report after careful reading. An article on "A case of Enlargement of the Eye following the entrance of steel into the eye" describes the panophthalmitis followed by bursting and shrinking of the eyeball (p. 601).

In another paper he described "two cases of ossification of the lens with luxation through the pupil." These cases are of some interest. He extracted the lenses and the patients did well (p. 608). An article on amaurosis associated with inordinate thirst was probably written by Jameson (p. 664).

In the American Medical Recorder of Philadelphia (vol. XII, p. 340) we find an interesting account of the successful removal of "An encysted tumor of the orbit."

In discussing a letter on "Ophthalmia in the Philadelphia Alms House," written to Dr. Rush, Jameson considers the question of the endemic or contagious character of the ophthalmia, excludes the latter view, and attributes the very severe disease to the vitiated state of the atmosphere. He states that "we do not as a general rule of practice bleed sufficiently in cases of ophthalmia."

John Mason Gibson.

John Mason Gibson in 1832 published in Baltimore (W. R. Lucas) a "Condensation of Matter upon the Anatomy, Surgical Operations and Treatment of Diseases of the Eye, together with remarks. Embellished with twelve lithographic plates, illustrative of the anatomy, operations, and morbid appearance."

I am unable to give any details of this author's life. I find that he was admitted into the Faculty in 1825, and that his name appears as late as 1848 in the list of members with the title of L. M. In the next succeeding list published in 1853 Gibson's name is missing.

In the preface he tells us that his book is an "attempt at collecting the best matter on diseases of the eye." That "diseases casual to vision are many and frequently met with in this country; the curative practice has not been sufficiently inculcated in our universities, by impressing upon the mind of the student where and when the importance and great nicety of judgment are requisite in the treatment of them, and that by inadvertent and mal-practice the victim may grope through his existence here in the valley of darkness."

The work is one of compilation, "being made up of extensive quotations from the classical writers of the day." He claims originality only in the construction of his plates, and so far he is certainly correct, for the drawings are quite unlike anything seen in nature.

The arrangement of the work is very curious. The chapters follow in this order: Anatomy of the eyeball, cataract, ophthalmia, corneitis, iritis, choroiditis, retinitis, inflammation of the lens and its capsule, ulcers of the cornea, opacities of the cornea, ptergium, prolapse of the iris, extirpation of the eyeball, extraneous bodies, diseases of the lachrymal apparatus, under which is included entropium, ectropium, epiphora, encanthus, injury of lids, ophthalmia tarsi and fistula lachry-

malis, looking very much as though the subjects had been drawn haphazard from a grab-bag.*

At the end of several chapters the author adds original remarks. When we consider the very excellent and systematic work which Frick had published, nine years previously, as well as such other works from which copious quotations are made, it is quite inexplicable why Gibson showed such disregard of systematic classification, or indeed what purpose he had in publishing the work at all.

DR. JOHN HARPER.

Dr. David Meredith Reese, in his American edition of Cooper's Dictionary of Practical Surgery, first published in 1832, tells us, under the section of cataract, that "one of the most successful operators in this country is Dr. John Harper, of Baltimore, and he seldom adopts any other operation than this (laceration of the capsule and lens substance), which he repeats as often as necessary on the same eye." I have given myself great pains to obtain some information concerning this "successful operator," but his memories appear to have been completely effaced. One single reference is to be found, a short obituary notice. In the Maryland Medical Recorder (vol. II, 179) there is a notice of the death of four members of the Medical and Chirurgical Faculty of the State of Maryland. One of these reads as follows: "Died in the month of January, 1831, Doctor John Harper. Doctor Harper was a native of Ireland, and graduated at Glasgow. He was well-known as an oculist." The title of oculist appears to have been elevated to the dignity of a special practice of medicine within a few years. For Harper was a member of the Faculty, and not of the despised class of oculists mentioned in the beginning of this paper.

WILLIAM ALEXANDER CLENDINEN.

Wm. A. Clendinen graduated in the medical department of the University of Maryland in 1840, a classmate of Dr. G. W. Miltenberger. He died of cholera at New Orleans in 1849, having been seized with the disease while dissecting a victim of the epidemic. After his graduation he traveled extensively, devoting his time to the study of medicine. In the Boston Medical and Surgical Journal in 1847 we find several papers which were translations from the work of Prof. Desmarres. In his letter to the editors he tells us that the extracts are part of a "translation upon which he is now engaged"; he expresses his gratitude to Prof. Desmarres, who "has entrusted to him an onerous but useful task, one which from (his) connection with him (he) may be able to perform advantageously to readers of the English language." In the second article, published in the same journal, we find after the name of the author, "Chef de la clinique oculaire." There were in all but three articles, and the promised book never made its appearance, perhaps on account of the untimely death of the author.

* Not a few important subjects are entirely ignored, viz. errors of refraction, strabismus, etc.

OTOLOGY.

The early contributions to Otology in Baltimore were very few. Two names deserve recognition; the first of these is that of the renowned surgeon,

NATHAN RHYNO SMITH.

It is beyond the province of this paper to give a biographical sketch of Dr. Smith. A very complete sketch can be found in Dr. Cordell's History of the University of Maryland. He was born in 1797 in New Hampshire, graduated as A. M. in 1817, and as M. D. in 1823 in Yale College. He was professor of anatomy and surgery in the University of Vermont in 1825, soon leaving to occupy the chair of anatomy in the newly organized Jefferson Medical College of Philadelphia. From 1827–29 he was professor of anatomy in the University of Maryland, and from 1829–38 he occupied the chair of surgery at the same university, leaving this for three years to occupy the chair of theory and practice in the Transylvania University. He resumed it again in 1841 and held it until 1869.

One of the earliest writings of this prolific worker was the translation of a treatise on the ear from the French of Saissy,* with additions by the translator on diseases of the external ear. This book was published in Baltimore in 1829, and was, so far as I can learn, the second book on diseases of the ear printed in America, the first having been an American edition of Saunders on the eye and ear in 1821.

Saissy's work was "highly esteemed in France," and was one of the important factors in the revival of modern otology. In his preface Dr. Smith tells us that "a concise manual on the diseases of the ear is an acknowledged desideratum in our medical literature. No sufficient work on the subject has ever been issued from the American press. With a view to supply this deficiency (he has) translated the following pages from the French of Saissy . . . corrected and enlarged by its author, and after his death published in 1827 by his friend Montaion, etc.

"It embodies the excellencies of Saunders, Cooper, Leschevin, Maunoir, Itard and Alard . . .

"The attention of the reader will be particularly occupied with Saissy's excellent method of injecting the ear through the eustachian tube. We very well know how frequently the external ear is obstructed by its own secretions, free as is their egress from this cavity. The internal ear is also lined with a membrane which furnishes an excrementitious fluid. It can escape only by the narrow channel of the eustachian tube. How frequently then must it be delayed in the cavity of the tympanum and mastoid cells, giving rise to any degree of mischief."

To render "the work more complete and useful to the medical pupil (he has) added a brief supplement on diseases of the external ear. On this score, however, (he) claims nothing, as (his) addition is made up of commonplace principles and

*An Essay on the Diseases of the Internal Ear, by J. A. Saissy, M. D., translated from the French by Nathan Rhyno Smith, M. D., Professor of Surgery in the University of Maryland, with a supplement on Diseases of the External Ear, by the translator. Published by Hatch & Dunning, Baltimore, 1829.

precepts subjoined merely for the purpose named above." He concludes: "I have, it is true, for perforating the tympanum devised a new instrument which I trust will be useful."

The part of the book most interesting to us is the supplement on diseases of the external ear, covering about twenty pages. These chapters are written in the most concise and simple manner and cover most of the inflammatory affections of the auditory canal, congenital deformities, injuries as well as the treatment of foreign bodies, insects and indurated wax in the auditory canal.

He describes his method of inspecting the canal as being "best accomplished by placing the head in such an attitude as to suffer the sun's rays to enter the meatus and impinge upon the tympanum. To effect this, the operator must seize the external ear, and drawing it outward from the head, extend and straighten the cartilaginous part of the meatus. I have been able to inspect the ear more perfectly by introducing, at the same moment, a steel director, with its groove toward the meatus and its convex side pressed firmly against the anterior walls. The passage is thereby straightened and expanded. If there be no wax present the tympanum will be seen of a pearly white color and concave."

The little instrument which he devised for perforating the membrani tympani is a minute trephine, by revolving which a circular piece of the drum was excised. His object in constructing this instrument was to obtain a larger opening, for he tells us that in two instances in which he had occasion to perforate the membrani tympani he found the beneficial effects of the operation soon to cease in consequence of the speedy closure of the artificial opening. The Maryland Medical Recorder of 1829 reviews the work extensively, the review covering twenty pages. The review is by no means flattering.

It is very apparent that Dr. Nathan R. Smith must have been deeply interested in the subject of the diseases of the ear. He had an extensive practice in diseases of the ear, and also of the eye, though none of his publications deal with the latter organ. Dr. Theobald has recently found a drawing of a knife designed by Dr. Smith to slit the lachrymal canal.

Dr. Joshua I. Cohen.

Dr. Cohen, born in Maryland in 1800, graduated at the University of Maryland in 1823, having been a student in Dr. Nathaniel Potter's office, and soon after devoted himself to the study of diseases of the ear. He was an intimate friend of Dr. George Frick, the oculist, and, like his friend, had wide interest in science beyond the domain of medicine. He thus for a time became professor of mineralogy in the academic department of the University of Maryland. He was much interested in the Medical and Chirurgical Faculty of Maryland, was its treasurer from 1839 to 1856 and president from 57–58. He was also much interested in the Maryland Academy of Sciences.

He practiced until about 1851, devoting himself almost exclusively to otology. His reputation as an aurist must have been quite great, for we read in Reese's American edition of Cooper's Dictionary of Practical Surgery (2nd ed., vol. 2, p. 73, under the heading "Ear"): "In the United States there have been a few surgeons who have distinguished themselves by their success in the treatment of diseases of the ear.

"Dr. Cohen of Baltimore and Dr. Dix of Boston have for several years directed their particular attention to diseases of the internal ear, and to the investigation of the abnormal condition of the tympanum and eustachian tube in cases of deafness. These gentlemen have employed condensing apparatus for administering the air douche through the eustachian tube after the plan of Kramer and others. By the air and also by the water douche, these gentlemen have acquired great tact in the diagnosis and treatment of obstructions in the tube and upon the tympanum."

In 1840 he established, in connection with his friend, Dr. Samuel Chew, an eye and ear institute in Baltimore, in which Dr. Chew had charge of the eye department. Dr. S. C. Chew has had the kindness to inform me that this association between Dr. Cohen and his father lasted for a short time, as his father's appointment to a chair in the University of Maryland compelled him to withdraw from it.

Dr. Cohen was one of the earliest, perhaps the first, aurist in this country. He has left us, however, but one publication which pertains to diseases of the ear. It is entitled "Post-mortem Appearances in a Case of Deafness."* The paper is very short, but is written in the most scientific manner.

In a note written by the editor of the Journal we read that this "valuable communication was read before the American Philosophical Society, at a recent meeting, and is noticed in the proceedings of that body. It has rarely happened that opportunities have been embraced for examining into the condition of the organ of hearing in cases of deafness, or that they have fallen within the observation of an investigator so competent as the author of this paper." The case was that of a patient who died of phthisis. The brain and seventh pair of nerves were examined carefully, but no changes observed. The lower part of the skull was then removed and the ear examined in minute detail.

In the right ear he found the drum-head dull and dark in appearance, irregularly thickened and retracted as a whole, thus diminishing the cavity of the tympanum. The tympanum itself was filled with muco-fibrous membranes passing from the membrani tympani to the posterior walls, presenting a cellular structure. These were carefully divided, exposing the tensor tympani muscle, the tendon of which was found to be of unusual shortness and attached to the handle of the hammer throughout its whole length, thus drawing the bone and the membrani tympani to within a line of cochlear process. Interesting irregularities in the ossicula were noted. The malleus was normal. The incus was undeveloped, diminutive in size. The stapes was wanting with the exception of the base, which was held in place by the circular ligament. The depression of the oval window was filled with membranous tissue, which likewise covered the fenestra rotunda. The tensor tympani muscle was strongly developed, its tendon short and thick as mentioned above. The stapedius muscle existed, but there was no tendon.

In the left ear the membrana tympani was found to have been entirely destroyed, with the exception of a very thin slip at the anterior inferior edge. The tympanum contained a

*Amer. Med. Intelligencer, July 1841 to July 1842, p. 226 (Vol. 1).

quantity of yellowish fetid matter, and its lining membrane was completely disorganized. The union between the ossicula was slight, owing to the general disorganization of the ligamentous and muco-fibrous connections. The incus was in place, but the handle of the malleus was depressed. The stapes was not bound down in the oval window, for the annular ligament was entirely destroyed. The tendon of the tensor tympani was disorganized and that of the stapedius destroyed; the whole condition of the tympanum showed a recent active suppuration which did not confine itself to this part; the vestibule was penetrated as well as the mastoid cells; the latter were covered with pus. The cochlea and one of the semicircular canals were examined a day or two subsequently, but there was nothing remarkable about them at this time worthy of note.

Inquiry about the patient led the author to conclude that the faculty of hearing in the right ear had been entirely wanting or was very much impaired for many years.

Though he seems to regard the condition of the right ear to have been congenital, his critical analysis is as acute as the observations themselves are accurate. He cites cases of Morgagni in which membranes filled the cavity of the tympanum, and another in which there was immobility or contracture of the muscles. At the conclusion of his paper he says: "In the case described in this paper, does not the absence of every part of the stapes, with the exception of the base, liken it to the osseous operculum found in the bombinatores, land salamander, and cæciliæ; of the effect of which, in the communication of sonorous undulations, I have already spoken?" I am unable to find any other reference to this paper on undulations and do not know whether it was ever published.

This sketch must be brought to a close. Is it necessary to remark what any careful reader must have observed, that there were physicians in Baltimore in the first half of this century who labored faithfully and well in Ophthalmology and Otology? Their contributions were among the earliest and most important in this country.

In conclusion I desire to express my thanks to Dr. G. W. Miltenberger, to Dr. John Morris and to Dr. Eugene F. Cordell, who furnished me with important notes, as well as my indebtedness to Dr. John R. Quinan's "Medical Annals of Baltimore," and Dr. Cordell's "History of the University of Maryland."

JOSEPH FRIEDERICH PIRINGER: HIS METHODS AND INVESTIGATIONS.*

BY HARRY FRIEDENWALD, A. B., M. D., *Baltimore.*

"Joseph Freiderich Piringer was born, March 31st, 1800, in Klein-Zell in Upper Austria. He studied medicine in Vienna; he afterwards turned his attention to ophthalmology in 1824. He was at first an assistant of Jaeger, then for three years of Rosas (beginning in 1825). He next obtained the extraordinary professorship of ophthalmology in the medical school at Gratz. Here he founded a much-needed eye hospital, an institution out of which the ophthalmic department of the Allgemeines Krankenhaus gradually developed through his energy. Here he held a high position and was active as a teacher until 1860. His clinical and experimental studies on blennorrhœa were made at this hospital. He wrote a work on blennorrhœa which was awarded a prize by the German Society of Physicians in St. Petersburg. Piringer has the merit of having shown that intentional infection of blennorrhœic secretion in pannus leads to clearing of the cornea. He founded a reputation upon this which extended far beyond the boundaries of Germany. He was for many years attached, as visiting physician, to the Elizabeth Hospital and to the City Asylum for the Aged, and published several other articles up to the time of his death, Sept. 22nd, 1879, including: Ueber Veratrinbehandlung des acuten Gelenk-Rheumatismus; Die Behandlung der Variola mittelst Iodtinctur; Die richtige Pflege der neugebornen und kleinen Kinder; Studien ueber die Mortalitäts-Statistik in Graz."†—(From Hirsch's Biographisches Lexikon, 1886, vol. IV, p. 575.)

Piringer's work on blennorrhœa‡ was awarded a prize, and the earlier works in ophthalmology refer to his studies, especially to his investigations in the treatment of pannus.§ Arlt devotes some space to a summary of the work on blennorrhœa.* But more recent works in ophthalmology make no mention of these important studies and their no less important results; in works on bacteriology I do not find his name, nor even in works on gonorrhœa, such as that of Finger. Piringer's work was of such importance that it does not deserve to be thus forgotten.

The absence of any other references in ophthalmic literature leads me to believe that Piringer's studies in ophthalmology were confined to the subject of inquiry contained in the work above mentioned. This is the more readily understood when we read in his preface "that his untiring efforts during fifteen years were given up to the solution of the natural laws governing blennorrhœas of the eye, as far as this lay in the limited powers of a single individual." We may aptly apply to Piringer the words, "therefore by their fruits ye shall know them." Judged by this standard, Piringer stands out boldly as one of the greater luminaries of medical science in the first half of this century.

In order to properly estimate the importance of his investigations it will be necessary to examine into the state of knowledge of the subject previous to 1840.

In 1780 a treatise on diseases of the eye appeared, written by George Chandler, surgeon, of London, in which he gives this account of "venereal ophthalmy." He divides it into two varieties, the translative and the symptomatic.

"The translative ophthalmy begins with a copious discharge of a sebaceous humor of a yellowish white color, and is known by the tumor, lividness, sharp and lancing pain of the sclerotica, the

* Read before the Johns Hopkins Hospital Historical Club.

† I am indebted to Professor Wm. H. Welch for having called my attention to this account of Piringer's life.

‡ Die Blennorrhoe am Menschenauge. Graz, 1841.

§ Oest. Med. Jahrb., 1838, and chapter 8 of the monograph.

* Klin. Darstellung d. Krankheiten d. Auges. Vienna, 1881. pp. 35–37.

cornea at the same time being as it were depressed within a pit. It frequently follows soon after an injudicious stoppage of a gonorrhœa, the venereal virus being translated into the eye. It has also been observed that a gonorrhœa which before seemed incurable has, upon the coming on of this ophthalmy, suddenly vanished; and in like manner the ophthalmy has gone off upon the return of the gonorrhœa . . . The symptomatic remits towards morning, never turns into a chemosis, the morbid matter does not change place, the pains are milder, it is removed when the lues is cured, and is also attended with less danger."

He admits that "sometimes a venereal ophthalmy has been produced by the immediate application of the virus to the eyes."

Concerning the treatment he tells us that—

"The venereal ophthalmy in general is subdued and its poison extinguished by mercury, but it should never be applied to the eyes. The patient should be bled and purged, and his eye washed continually with brandy and water, etc. It is necessary also to evacuate the virulent matter collected in the cellular texture of the sclerotica and eyelids by slight incisions of each membrane; an ichor, very like that of gonorrhœa, will be discharged."

This was the older view.

About the beginning of this century a remarkable development occurred in ophthalmology. Beer and Schmidt in Vienna, and Himly, Langenbeck, Graefe the elder, and others gave a new impetus to the scientific study of this branch of medicine. This was followed by the publication of a number of important treatises and monographs in ophthalmology, principally in Austria and Germany, but also in France, England and our own country. I shall pass these by and take up the subject as we find it in the third edition of one of the most important and valuable treatises on "Diseases of the Eye" written in the first half of this century, that of William Mackenzie of Glasgow. I take the third edition as it appeared in 1840, one year before Piringer's work. Here we find under the section of ophthalmia in new-born children, that "there is reason to suspect that this disease is not unfrequently an inoculation of the conjunctiva," etc., and "that therefore it may often be prevented by carefully washing the eyes of the infant with tepid water," etc.; secondly, "that the purulent ophthalmia of infants in its worst form is the result of the application of gonorrhœal matter, is generally admitted."

Concerning gonorrhœal ophthalmia he tells us that—

"Different views have been entertained of the purulent inflammation of the conjunctiva which is frequently found to attend or succeed gonorrhœa. First, this ophthalmia has been ascribed to inoculation with matter from the urethra; secondly, it has been supposed to be metastatic; and thirdly, it has been considered, at least in certain cases, as an effect owing to irritation merely, without either inoculation or metastasis. It is quite possible that there may be three such varieties of this ophthalmia. The existence of the first I consider beyond all doubt; but the second and third are somewhat problematical."

His description of the first form is quite clear, and in several cases which he reports the infection is definitely proved. He did not succeed in separating gonorrhœal from what was called Egyptian ophthalmia, excepting as differing in degree. This distinction was not definitely made until about ten years later (by Bendz). Mackenzie's criticism of the views entertained by some eminent ophthalmologists is so interesting that I shall quote the entire paragraph:

"Dr. Vetch tells us that in a soldier in a very advanced stage of Egyptian ophthalmia he attempted to divert the disease from the eyes to the urethra, by applying some of the matter taken from the eyes to the orifice of the urethra. No effect followed this trial. It was repeated in some other patients, all laboring under the most virulent state of the Egyptian disease; and in all the application was perfectly innocuous. But in another case, where the matter was taken from the eye of one man laboring under purulent ophthalmia, and applied to the urethra of another, the purulent inflammation commenced in 36 hours afterwards and became a very severe attack of gonorrhœa. From the result of these experiments, Dr. Vetch, while he admits that gonorrhœal matter taken from one person and applied to the conjunctiva of another will excite a highly purulent ophthalmia, regards himself justified in no longer admitting the possibility of infection being conveyed to the eyes from the gonorrhœal discharge of the same person. He adds that the impossibility of this effect was rendered decisive by an hospital assistant who, with more faith than prudence, conveyed the matter of a gonorrhœa into his eyes without any affection of the conjunctiva being the consequence. It is remarkable that Dr. Guillie has fallen into the same error of reasoning as Dr. Vetch, only that his negative experiments have led him to the very opposite conclusion. He applied the matter taken from the conjunctiva of one patient to the urethra of another; no effect followed, and hence he concludes that the notion of some regarding the propagation of puro-mucous inflammation from one mucous membrane to another in different individuals, is unfounded."

Mackenzie, however, devotes considerable space to the discussion of gonorrhœal ophthalmia from metastasis, though nothing could be better than his statement that writers had adopted these views "with too little hesitation and appear not to have sufficiently investigated the probability of the ophthalmia arising rather from inoculation than from metastasis." He goes on to enumerate "the causes of the suppression of the gonorrhœa, to which the rise of metastatic gonorrhœal ophthalmia is attributed," and cites an illustrative case from a French writer. It is evident that Mackenzie had little confidence in the existence of this form of gonorrhœal ophthalmia.

A third form of gonorrhœal ophthalmia without inoculation or metastasis is described: "an alternation has been observed between the two diseases; that is to say, when the gonorrhœa came, the ophthalmia went, and *vice versa*." Mackenzie says that the cases reported "show the diversity which exists in opinions entertained regarding the ophthalmiæ which in some individuals are found to attend gonorrhœa, or to alternate with this disease"; and that "it is quite evident that the ophthalmiæ which have been observed to do so are far from being uniform," that some are probably ophthalmia tarsi, others catarrhal ophthalmia. What is most important is his statement that "it may fairly be doubted whether there is any connexion between diseases of urethra and that of the eye, farther than that they occurred in the same individuals, while the occurrence of both might be attributed to a susceptibility for disease arising from peculiar or debilitated constitutions." We are somewhat surprised to see this statement followed by such a one as this, that "Swediar's hint to employ the bougie in cases of ophthalmia alternating with gonorrhœa may probably be found of use; it is evident, however, that this remedy cannot be trusted alone, but that the ophthalmia must be treated according to

the particular symptoms it presents, not according to the conjectural notions entertained regarding its origin."

Concerning Egyptian ophthalmia Mackenzie says:

"I think it probable that the ophthalmia which attacked the British and the French armies in Egypt was an atmospheric puro-mucous conjunctivitis [described at another place as "excited by exposure to atmospheric alternations"], but that it afterwards degenerated into a contagious, perhaps infectious disease; that is to say, that it was propagated by actual contact of the discharge, and perhaps by miasmata from the eyes floating through the air."

Mackenzie's views may fairly be taken as the most advanced of this period and will serve as the proper point from which to view the investigations of Piringer.

It should be mentioned here that many subsequent writers held on tenaciously to these and older views for many years after Piringer's discovery had been made.

We may first direct our attention to Piringer's experiments in curing pannus by producing acute purulent ophthalmia. This method, we are told, was first suggested by Friederich Jäger* some time during the second decade of this century, but little or no attention had been given it before Piringer's experiments were made. He tried the method in more than sixty cases, using the pus of various kinds and stages of purulent ophthalmia of both adult and new-born. All of his cases were improved and not one was injured by the treatment. In the great majority of cases the cure was complete, so that no sign of the former disease could be discovered, and permanent. He therefore recommends this method of curing pannus in these words: "After so many highly successful experiments and observations, the inoculation of the blennorrhœa for the cure of pannus is no longer a doubtful measure which requires great courage, but an excellent method (ein grossartiges Mittel)." This method remained in use for many years and is still being applied in a modified manner. The modification consists in the use of jequirity to produce a purulent ophthalmia instead of blennorrhœic matter.

It was in the study of this method of treatment that Piringer made his investigations as to the nature of blennorrhœa and the contagious property of the secretion. For this purpose his experiments were varied in every conceivable manner. Most of the experiments were made upon eyes which were already diseased, but these were controlled by other experiments upon amaurotic eyes with perfectly healthy conjunctiva, or by the accidental infection of normal eyes.

After describing the various forms of purulent inflammation of the conjunctiva he takes up the causes. Under this head he discusses the question as to the production of gonorrhœal ophthalmia by metastasis. In the course of fifteen years he had never seen a case of suppression of a gonorrhœa with an outbreak at another point, excepting in the neighboring tissues. Many physicians of great practice had likewise never seen true metatastic gonorrhœal ophthalmia. He denies the occurrence of such a form of disease, and characterizes it as a very pretty fable which one after the other has been telling in the best of faith. Gonorrhœal ophthalmia is always due to the transference of infectious material directly into the conjunctiva!

In a similar manner he disproves the existence of a consensual form of gonorrhœal ophthalmia, supposed to be due to a sympathetic connection between the affected parts.

Gonorrhœal ophthalmia is known to be more common toward the end of the primary affection than during the period of its greatest virulence. He explains this very properly on the ground that when the discharge is great much care is used in cleansing the fingers. It is after the discharge has become scant that patients become careless. The right eye is usually the one first affected, because most patients are right-handed.

His experiments proved that the generally accepted view that the transference to the eye of gonorrhœal pus results in a simple conjunctivitis or a mild purulent ophthalmia, and only rarely in a severe inflammation, is false. On the contrary, he asserts that the result is always a purulent ophthalmia of a severe degree. The only exception occurs in those cases in which early treatment is successfully applied.

The contagious character of the secretion of ophthalmia neonatorum had been looked upon as ridiculous;* when Juengken stated that a nurse had developed severe purulent ophthalmia during the night of the same day when she had infected them with a sponge that had been used to cleanse the eyes of an infant with ophthalmia, it was laughed at. No one can deny the infection in his own cases, because the material was carried across the street into another house and yet the result was always the same.

His experiments on the production of purulent ophthalmia embrace almost one hundred eyes, and this does not include a number of accidental infections which were observed with the same care. The matter with which infection was produced was obtained from eyes affected with different forms of purulent ophthalmia as well as from genital blennorrhœa. All ages and both sexes were found to be equally predisposed.

One of the most interesting chapters is the one on the character of the blennorrhœic contagion. Though the pus from the conjunctiva causes infection when applied to the eyes of human beings, he was unable to produce any result when he applied it to the eyes of such animals as dogs, cats, hogs, and various birds. Others had claimed that they had produced effects in such cases. Our present knowledge of the immunity of animals to gonorrhœa shows that Piringer was not deceived.

The granulation from which the pus is secreted carries the contagious property with it, as was shown in 1823 by Werwick, who experimented upon two nurses. Piringer never made any experiments with granulations because it lacked any practical bearing.

*In Hirsch's History of Ophthalmology, p. 441, we read that Ludwig reported the successful use of this method at the hands of a friend, probably Friederich Jäger.

Wharton Jones (in a manual on Ophth. Med. and Surg. published in 1847) states that Dr. Henry Walker was the first to suggest the method (Edinburgh Medical and Surgical Recorder) in 1811. I am unable to verify this citation.

*The faith with which this opinion has been held can be measured by the much-quoted citation of Dr. Vetch, referred to above. Piringer likewise quotes Adams, who rubbed his eyes with a finger smeared with purulent matter without producing ill results, as did Van Sevenoeck and Kriebel. Morburgo smeared the eyes of 300 soldiers without effect.

The pure lachrymal fluid of a blennorrhœic eye was found not to possess contagious properties. An experiment is related in which lachrymal fluid collected upon a camel-hair brush did not produce infection, while the purulent secretion gathered a few days later did.

The question of contagion *par distance* (per miasma) was studied in his hospital, where numerous blennorrhœic patients were placed beside those unaffected, in small wards. He was able to prove definitely that such contagion does not occur and that the cases in which it is suspected can always be traced to direct infection.

The vapor of drying secretion has no power of infection. Even those secretions which are perfectly serous leave a residue when evaporated, and it is this which still retains the contagious properties.

The fluid secretion when kept for three or four days, even without evaporation, is no longer contagious.

If a finger is covered with blennorrhœic secretion and washed immediately in clear fresh water and well dried, it will cause no infection if rubbed over the conjunctiva.

Blennorrhœic matter may be smeared over the eyelids if the palpebral fissure is kept firmly closed by adhesive plaster, and no infection will occur. In order to produce infection the secretion must reach the conjunctiva.

The secretion of acute blennorrhœa of a moderate degree is just as contagious as that of the most severe. Such pus is capable of producing infection even when diluted with 50 or 100 parts of water. The pus of a very mild purulent ophthalmia and of chronic ophthalmia has much less power of infection.

As long as there is any secretion in any case of purulent ophthalmia, whether mild or severe, so long the case is contagious. When the secretion ceases, though there may be considerable congestion and photophobia, the infectious properties are lost.

The properties of infection inherent in the secretion do not vary in different seasons of the year nor under changed atmospheric condition, whether exposed to the glare of the sun or to the cold of the winter. Experiments to determine this may appear meaningless to us, but we must not forget the opinions prevalent in Piringer's time.

Numerous experiments were made to determine the effect upon the contagious properties of the pus when separated for varying intervals of time from the human body. Thus the secretion was collected upon a camel-hair brush and allowed to become as dry as possible ("dry as glass") in from three to six hours. If then applied to the conjunctival sac, but not allowed to soften in the tears, no infection occurred; if softened in the tears, or previously in water, infection was sure to occur.

In seven cases the secretion which had been allowed to dry for thirty-six or forty hours in the open air had lost all contagious property, but in two in which the secretion had been allowed to get dry thirty-three or thirty-four hours in the room and was then softened it produced very severe ophthalmia blennorrhœa. Pieces of linen smeared with fresh blennorrhœic matter and given to a pannus patient to wipe his eyes produced infection, but if the cloth had been dried in the air for several days, the patient could use it about the eyes without harm, and the secretion when scraped off the cloth and inserted into the conjunctival sac produced infection only when it was less than thirty-six hours old. On the other hand, if the secretion was placed in a vaccine case and hermetically sealed it retained its contagious property for forty-eight hours, but lost it entirely when kept for three days. The pus from a case of ophthalmia neonatorum produced infection when kept in this way for two and a half days. Piringer therefore concludes that "*blennorrhœic pus loses its contagious properties as it becomes older, and gradually dies in from twenty-six to forty-eight hours, possessing no more organic life when three days have passed.*" It is upon the discovery of this fact that Piringer bases important rules of prophylaxis, and explains the relative infrequency of gonorrhœal ophthalmia among the common people.

Piringer studied carefully the length of time intervening between the entrance of the infectious material and the first signs of the developing disease, the period of incubation. This he found to vary according to the manner and the amount of the infection, as well as according to the individual peculiarity of the patient. The higher the degree of inflammation, the more rapidly did its pus infect. Thus the pus from a very severe ophthalmia produced infection in from six to twelve hours, or at most in thirty-six hours; while that of very mild blennorrhœa might require sixty to seventy hours, and that of a case of chronic blennorrhœa seventy-two to ninety-six hours.

The secretion of the second stage of acute ophthalmia acts more slowly than that of the first; thus the pus from a case of very acute blennorrhœa in which the secretion was markedly diminishing may take sixty hours to produce the first symptoms.

If the secretion while still warm is immediately transferred from an acute blennorrhœa, but six or eight hours are required for the first signs to show themselves, and in twelve or eighteen hours the disease is fully developed. In this respect the conjunctiva responds more rapidly than the genital mucous membrane. The longer the secretion is kept before it is placed in the conjunctiva, the less rapidly does it act. The rapidity likewise varies with the amount of secretion brought into the eye.

Piringer gave the prophylaxis of gonorrhœal ophthalmia his careful attention and the results were very important. Can the inflammation be aborted after the infectious material has once reached the conjunctiva? Several cases in which the very early and continued application of ice compresses produces this effect are described. In order to determine whether the infectious material can be removed and the outbreak of the disease prevented, several series of experiments were made. The method which required the application of very strong solutions of bichloride of mercury, concentrated acetic acid, etc., appeared to him too severe, and in the few cases in which it was tried the desired result was not obtained.

On the other hand he found that washing out the eye with water after the contagious material had been inserted prevented the development of the disease, provided that it is done sufficiently early. These experiments were made on three patients. In the first case he touched the eye with a large quantity of matter from a newborn babe, cleaned it out after a minute with a sponge dipped in cold water, and had cold water applications made for ten hours. No inflammation resulted.

Four days later he varied the experiment by allowing the matter to remain two minutes and again no inflammation occurred. After another interval of four days the experiment was again repeated, the pus remaining in the eye for three minutes and the result was again the same. After another interval of five days pus was inserted and allowed to remain in the eye for five minutes; no cold applications were made; the result was a violent inflammation. This patient had pannus, which was cured by the treatment.

In order to try these experiments upon a perfectly normal conjunctiva he selected the eyes of an amaurotic beggar whom he paid for these privileges. In this case he found that no disease resulted if the blennorrhœic matter was washed out within three minutes after its entrance into the conjunctival sac and cold water applied. In three minutes any one who has accidentally infected his eyes can obtain fresh water with which to wash them!

In order to determine whether the washing out of the conjunctival sac would accomplish the same result without the use of ice applications, he infected the eye of a girl suffering with pannus with blennorrhœic matter and washed out the conjunctiva in three minutes. Ninety hours later a severe ophthalmia developed. And in a second case the experiment again resulted in the development of a purulent ophthalmia, but not until the fifth day, and in this case the ophthalmia was of a milder character. For these reasons he considers the cold water application as essential to the prevention of conjunctival blennorrhœa after infection has occurred. Many other questions are considered by Piringer, but this review embraces the essential points, and will, I trust, suffice to show its importance.

The care, the true scientific spirit, the accuracy of observation and experiment, the unbiased search for truth, form marked characteristics of Piringer's work. And yet he is so modest as not to claim that his results are "the absolute truth," though they are his own firm convictions; experiments and observations, he tells us, by other physicians at other times and places, made with care and without prejudice, are needed to confirm them as well as to clear away any errors. His experiments were arranged carefully and judiciously to determine the character and the attributes of the contagion, the time during which it acted, the gradual diminution of its powers to the point when they were entirely lost, the intensity of the contagion, and the degrees of dilution which could be borne without loss of all contagious properties. Many of these questions were given their final solution by Piringer. He separated the fluid portions of the pus which could be evaporated, from the more solid matter which contained the contagious quality. He proved that the unknown cause of contagion was a something which had to be transferred in substance from one mucous membrane to the other, and which never acted at a distance, thus disproving the old miasmatic principles which were still current in those days.

In what is our knowledge to-day greater than that of Piringer, excepting that the active agent, the living micro-organism, the gonococcus, has been discovered, a discovery only made in 1879? That it was a living organism even Piringer surmised, for, as we read above, he speaks of it as "growing older," and "dying," and "possessing organic life." Let us not forget when it was that Piringer worked. Though micro-organisms had been discovered toward the end of the seventeenth century, and micro-organisms were assigned as the causes of numerous diseases during the eighteenth century, it was not until 1840 that Henle first established the germ theory of disease upon the solid foundation of logic and fact.

One aspect of Piringer's experiments still deserves mention. Others had likewise made experiments with a view of transferring the disease which we have been considering; but most of these were unsystematic, few in number, and led only to confusion. Piringer's work was such as to give a definite solution to important questions, results which have stood the tests of time. His work was not in vain. His hopes were fulfilled that "the medical world would read not entirely without pleasure a number of results, whose collection in the field of practice had been made at the cost of much strain, great pains and many a sleepless night."

PROCEEDINGS OF SOCIETIES.

THE JOHNS HOPKINS HOSPITAL MEDICAL SOCIETY.

Meeting of May 3, 1897.

DR. THAYER in the Chair.

Hæmatomyelia from Gunshot Wound of the Cervical Spine.—Dr. CUSHING.

This patient, a young woman of 27 years, was brought into Prof. Halsted's service, November 6th, 1896, with the history of having been shot twice in the neck a few hours before entrance. Our chief interest centres in the injury produced by the missile, whose wound of entrance is situated here on the right side of the neck at the level of the cricoid cartilage. These skiagraphs show the point of lodgment of the bullet in the centrum of the sixth cervical vertebra.

The case illustrates some unusual features of one type of the so-called Brown-Sequard paralysis; the value of the X-rays in locating the offending missile; a practical recovery without operative interference; a residuum of symptoms resembling those of syringomyelia.

At entrance she was suffering from agonizing "pains" of a "pins and needles" character, especially in the arms, so severe she would cry out when they were touched. No radiating pains, such as are described in the meningeal form of hemorrhage, were present. There had been no loss of consciousness. Motor paralysis was complete on the right side below the level of Thorburn's "5th root group." On the left there was a brachial monoplegia up to this same segmental level, namely that for the deltoid biceps and supinator longus muscles whose ventral horn ganglion cells lie in the 5th cervical segment, as you may see by comparison with this

familiar table of Starr's. Respiration was purely diaphragmatic.

On the left side, up to the second intercostal space and down the ulnar side of the arm over an area which corresponds, as these diagrams taken from Starr and Kocher demonstrate, to the cutaneous sensory fields of the eighth cervical and first dorsal segments, there existed anæsthesia to pain and thermic stimuli, and a slight dulling of tactile or pressure sensibility. A segmental zone of similar anæsthesia was present on the side of motor paralysis across the chest and including most of the right arm.

There was a marked elevation of surface temperature, considerable hyperalgesia, and also a diminution of reflex activity on the paralyzed side. Loss of sphincteric control over the contents of the bladder and rectum was also present.

Restitution from these paralytic symptoms has taken place up to the excellent condition in which you see the patient at present. The motor paralysis disappeared gradually. On the tenth day, motion, as is usual, was first observed in a slight flexion of the thigh. Turner in his experimental work called attention to this. At the present time there remains evidence of the degeneration of the right pyramidal tract in Wernicke's residuary paralysis of the dorsal flexors of the foot, giving the late hemiplegic character to the patient's gait. Mann has recently called attention to this residuum of motor paralysis and has shown that it selects a certain muscle unit (bewegung einheit), this unit being that which lifts and shortens the leg in the first stage of walking, as can be demonstrated in this patient. Paralysis remains alone complete in some of the small muscles of the right hand, representing destruction of ganglion cells at the site of the lesion. There is an exaggeration of all the deep reflexes of both extremities. Myotatic contractions, as you see, are elicited in the muscles of the right arm by gentle blows on the tendons. Resolution of the sensory disturbances has been less complete. Slight anæsthesia to painful and especially thermic stimuli persists on the left.

A month ago there was present a peculiar distribution of areas of hyperæsthesia, over which cold produced pain without thermic sense, and the threshold for pain was so low to touch or pressure stimuli that the latter could only be elicited with difficulty by v. Frey's æsthesiometer. This is illustrated by these diagrams.

The lesion was presumably due, as in Mann's case, to an intramedullary hemorrhage, the occurrence of which, especially in the cervical enlargement, as emphasized by Thorburn, is not uncommon. It seems that not only is traumatic hæmatomyelia much more common here than elsewhere in the cord, but that there is a certain level in this enlargement giving symptoms corresponding to the "typus inferior" of Krause, in which intramedullary hemorrhage is most likely to occur. This list of cases, collected from the literature, tends to confirm this view of a site of predilection in the lower cervical enlargement for hæmatomyelia, and illustrates the fact that not only in cases attributable to acute flexions of the neck does it occur, as was believed by Thorburn, but also in those due to a great variety of traumatic causes. The hemorrhage also is apt to occur most extensively on one side of the cord, thus often producing the symptom complex of a Brown-Sequard type of paralysis.

That the hemorrhage also in these cases selects by preference the gray matter of the cord, presumably from the greater vascularity and less support given to the vessels there, is well recognized. Frequent note has also been made of cavity formation at the original nidus of hemorrhage, which is interesting in consideration of the association of the syndrome of syringomyelia which Minor and others have pointed out to be a common sequence of traumatic hæmatomyelia and which persists in this patient.

The case which Mann has recently reported in full with post-mortem examination of the cord, in the Deut. Zeit. f. Nervenheil. for 1896, and which clinically bears such close similarity to this case, illustrates many of these points very completely.

Dr. Thomas.—This case seems to me to be one of particular interest, and the Society is to be congratulated on having it so admirably presented. Those of you who have had the opportunity of examining a similar case will recognize the difficulty of making such an exhaustive examination. I know of no case in the hospital records that has been worked up so carefully as this. Dr. Cushing was kind enough to bring the patient to me on several occasions for examination, and I confirmed the results which he had already obtained. The patient's condition, after she had been in the hospital for some time, was, as Dr. Cushing has described to you, briefly as follows: there was weakness of the right leg and right arm. The muscles of the right arm were atrophied, and there were sensory disturbances, as shown in the charts. The wound in the right side of the neck was in such a position that the bullet might have injured the brachial plexus before entering the spinal column, and thus have produced the condition found in the right arm. An electrical examination of the paralysed and atrophied muscles showed that they responded normally to the currents. This was believed to indicate that the brachial plexus was not injured, and also that the paralysis was not due to destruction of the anterior horns from which the nerves forming this plexus arise. I do not believe that there can be extensive destruction of the anterior horns without the occurrence of degenerative atrophy of those muscles which receive their nerves from that segment of the cord. In the case of Mann, to which Dr. Cushing has referred, there was paralysis and atrophy of the muscles of the left arm. This was completely recovered from, but, at the autopsy, a lesion in corresponding anterior horn was found. This case is, as far as I know, the only one bearing on this point, and it does not seem to me to be conclusive. The lesion was limited to the eighth cervical segment, as shown by the fact that the eighth root was degenerated, while the seventh was not, and the first thoracic root was only slightly so. We know that each muscle is represented in more than one spinal segment, and it can be understood how a limited lesion in one segment need not necessarily produce degenerative atrophy to such a degree that it could be discovered during life. The autopsy on Mann's case was incomplete, in that neither the muscles nor the peripheral nerves were examined.

Dr. Cushing's case is of interest from so many standpoints

that it is impossible to discuss it fully. Unilateral lesions of the cord are not very common. In the dispensary we have the records of only two typical cases. I have at present under observation a very beautiful example of this form of paralysis. The patient is a woman whose right leg is paralysed and who shows the disturbances of sensation in the left leg which Mann has pointed out as characteristic, *i. e.* the sensory conduction for touch is undisturbed, whereas pain, heat and cold stimuli do not produce their normal effect. The paths of sensory conduction within the cord have been and are the subject of much discussion. Of late the anatomists and physiologists have been inclined to the view that the paths do not cross, but remain on the same side of the cord until they reach the medulla. Clinicians, on the other hand, from the study of the cases of unilateral lesion of the spinal cord, have held to the belief that the sensory paths decussate soon after entering the spinal cord. That there must be an anatomical basis for the constant occurrence of the classical symptoms of Brown-Sequard's paralysis seems to me to be self-evident. The burden of proof must rest upon the anatomists and physiologists.

NOTES ON NEW BOOKS.

Diseases of the Rectum, Anus and Contiguous Textures. By S. G. GANT, M. D. (*Philadelphia: F. A. Davis*, 1896.)

With the exception of two chapters on Cancer of the Rectum and Colotomy by Allingham, a recognized authority on these subjects, the work of Dr. Gant is not to be commended. The book contains nothing new, and there are many superior works on this subject. The two chapters in which the author seems to take special pride, one on auto-infection, the other on effects of railroading on diseases of the rectum, are not remarkable, and the first is unsatisfactory. That employes of railroads suffer much from constipation and hemorrhoids as a result of their occupation may be true, but the figures he gives can be handled in many ways. If we eliminate from the author's statistics all cases suffering from these two very common conditions, we have less than 1 per cent. out of over 170,000 cases seen in various hospitals suffering from other diseases of the rectum and anus; and 5.7 per cent. in the same number of cases does not seem to us an excessive number to be suffering from the two conditions mentioned. We believe that among the poor laboring class constipation is the usual condition found, and that figures more striking than these could easily be collected to show that constipation was one of the commonest symptoms in all hospital cases. We question whether the jarring of a railroad train will continually rock a stomach from side to side, produce a "sea-sick stomach," or whether it is sufficient to bring about by itself dilatation of the veins about the anus and rectum by causing a venous stasis, and we know that swelling of the feet in railroad travelers is not an ordinary condition, to say the least. The chapter on sodomy might better have been omitted or much abbreviated; such a book does not call for a psychological explanation of this practice. In speaking of the relation of pulmonary tuberculosis to fistula, the author states: "So we meet with two kinds of tubercular fistula—one as a result of localized tubercular ulceration with or without any lung complication; the other a fistula in persons who have lung trouble, due to the absorption of fat about the ischia, general debility, and abscess. In the feces of the first variety can be found the tubercle bacilli of Koch, while in the second variety they cannot be found unless the sputum containing them has been swallowed, and gastric digestion has been impaired to such an extent as not to destroy them or their spores." In either form the fistula is tubercular and the bacilli can be found without these coming from the sputum; and gastric digestion, even when normal, does not ordinarily destroy them, as we know from the large number of cases of primary intestinal tuberculosis which occur. We do not think that the following statements as regards auto-infection are true. On p. 271 we read: "Our Creator, however, foresaw all dangers and provided us abundantly with safeguards with which we can destroy or neutralize the poisons on the one hand, or throw them off on the other, as soon as they are formed"; and on p. 272, "Just so long, however, as the emunctories are working in harmony and perform their individual functions, and there is no lesion of the intestinal mucosa, all is well, and all poisons, no matter whether they are the products of decomposition or of bacterial action, will do no harm, for the reason that they are thrown into a special reservoir (the liver) where they are destroyed or neutralized and afterward discharged from the body."

The colored illustrations in this work are hideous and unnatural, and the woodcuts are not models of artistic excellence. Would it not be well for authors to cease to reproduce cuts of the most common and ordinary instruments, and of well-known positions and procedures in operations, which are quite valueless (*cf.* cuts on pp. 20, 21, 22, 25, 218, 219, 220). We dislike extremely to see the author's autograph on the outside cover, and the long list of societies to which he belongs following his name on the title-page. If the book possesses merit of its own, this sort of cheap advertisement does it no good; and it does not help the work in any case.

BOOKS RECEIVED.

Second Catalogue of the Library of the Peabody Institute of the City of Baltimore. Including the additions made since 1882. Parts I-II. A—D. 1896-97. 4to. Baltimore.

The Semi-Centennial of Anæsthesia. October 16, 1846–October 16, 1896. 4to, 95 pages. 1897. Massachusetts General Hospital, Boston.

Saint Bartholomew's Hospital Reports. Edited by S. West, M. D., and W. J. Walsham, F. R. C. S. 1896. Vol. XXXII. 8vo, 548 + 197 pages. 1897. Smith, Elder & Co., London.

Transactions of the American Pediatric Society. Eighth session, held in Montreal, Canada, May 25, 26 and 27, 1896. Edited by Floyd M. Crandall, M. D. Vol. VIII. 1896. 8vo, 243 pages. Reprinted from the Archives of Pediatrics. New York.

Medical and Surgical Reports of the Boston City Hospital. Eighth Series. Edited by G. B. Shattuck, M. D, W. T. Councilman, M. D., and H. L. Burrell, M. D. 1897. 8vo, 391 pages. Published by the Trustees, Boston.

King's College Hospital Reports. Being the annual report of King's College Hospital and the medical department of King's College. Edited by N. Tirard, M. D., F. R. C. P., *et al.* Vol. III. (Oct. 1st, 1895-Sept. 30th, 1896.) 8vo. 1897. 332 pp. Printed by Adlard & Son, London.

Twentieth Century Practice. An international encyclopedia of modern medical science by leading authorities of Europe and America. Edited by T. L. Stedman, M. D. In twenty volumes. Vol. XI: Diseases of the Nervous System. 8vo. 1897. 962 pp. William Wood & Co., New York.

Burdett's Hospitals and Charities, 1897. Being the year-book of philanthropy. By Henry C. Burdett. 12mo. 1897. 1018 pages. The Scientific Press, London.

Archives of Skiagraphy. Edited by Sydney Rowland, B. A., Camb. Fol. Vol. I, No. 4. April, 1897. The Rebman Publishing Co., Limited, London.

A Pictorial Atlas of Skin Diseases and Syphilitic Affections in Photo-Lithochromes from Models in the Museum of the Saint Louis Hospital, Paris. With explanatory woodcuts and text by E. Besnier, A. Fournier *et al.* Edited and annotated by J. J. Pringle, M. B., F. R. C. P. Part X, 1897. W. B. Saunders, Philadelphia.

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